Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings*

Raj Chetty, Harvard University and NBER
John N. Friedman, Harvard University and NBER
Emmanuel Saez, UC Berkeley and NBER

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Abstract

We develop a new method of estimating the impacts of tax policies that uses areas with little knowledge about the policy’s marginal incentives as counterfactuals for behavior in the absence of the policy. We apply this method to characterize the impacts of the Earned Income Tax Credit (EITC) on earnings using administrative tax records covering all EITC-eligible filers from 1996-2009. We begin by developing a proxy for local knowledge about the EITC schedule – the degree of “sharp bunching” at the exact income level that maximizes EITC refunds by individuals who report self-employment income. The degree of self-employed sharp bunching varies significantly across geographical areas in a manner consistent with differences in knowledge. For instance, individuals who move to higher-bunching areas start to report incomes closer to the refund-maximizing level themselves, while those who move to lower-bunching areas do not. Using this proxy for knowledge, we compare W-2 wage earnings distributions across neighborhoods to uncover the impact of the EITC on real earnings. Areas with high self-employed sharp bunching (i.e., high knowledge) exhibit more mass in their W-2 wage earnings distributions around the EITC plateau. Using a quasi-experimental design that accounts for unobservable differences across neighborhoods, we find that changes in EITC incentives triggered by the birth of a child lead to larger wage earnings responses in higher bunching neighborhoods. The increase in EITC refunds comes primarily from intensive-margin increases in earnings in the phase-in region rather than reductions in earnings in the phase-out region. The increase in EITC refunds is commensurate to a phase-in earnings elasticity of 0.14 on average across the U.S. and 0.58 in high-knowledge neighborhoods.

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I Introduction

Research on the impacts of tax policies on economic behavior has confronted two important empirical challenges. First, because federal tax policies often do not vary cross-sectionally, it is difficult to find counterfactuals that permit credible estimation of the policies’ causal effects (Meyer 1995, Saez et al. 2012). Second, many individuals may respond slowly to tax changes because of inattention to the tax code and other adjustment frictions (Brown 1968, Fujii and Hawley 1988, Bises 1990). This makes it difficult to identify steady-state behavioral responses using short-run comparisons before and after a tax reform (Chetty et al. 2011, Chetty 2012).

We develop a research design that addresses these challenges by exploiting differences across neighborhoods in knowledge about the tax code. Our method is based on a simple idea: individuals with no knowledge of a tax policy’s marginal incentives will behave as they would in the absence of the policy.¹ Hence, one can identify the causal effect of a policy by comparing behavior across cities that differ in knowledge about the policy but are otherwise comparable. We apply this method to analyze the impacts of the Earned Income Tax Credit (EITC), the largest means-tested cash transfer program in the United States, on earnings behavior and inequality. We exploit fine geographical heterogeneity across ZIP codes by using selected data from U.S. population tax records spanning 1996-2009, which include over 75 million unique EITC eligible individuals with children and 1 billion observations on their annual earnings. Our method uncovers significant impacts of the EITC on earnings behavior. The intensive-margin responses we document are masked in aggregate data and cannot be easily detected using traditional research designs because of their diffuse nature, potentially explaining why prior studies find mixed evidence of intensive margin responses to the EITC and other tax policies.

Our empirical analysis proceeds in two steps. First, we develop a proxy for local knowledge about the marginal rate structure of the EITC schedule.² Ideally, one would measure knowledge directly using data on individuals’ perceptions of the EITC schedule. Lacking such data, we proxy for knowledge using the extent to which individuals manipulate their reported income to maximize their EITC refunds by reporting self-employment income. Self-employed tax filers have a propensity

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¹As we discuss in Section 2 below, this equivalence holds in the absence of income effects. With income effects, our technique recovers compensated elasticities under the assumption that uninformed individuals believe that the tax credit is a lump-sum subsidy.

²Throughout the paper, we use the term “knowledge” or “information” about the EITC to refer to knowledge about the program’s marginal incentive structure rather than awareness of the program’s existence. Surveys of low income families and ethnographic interviews show that most EITC-eligible individuals are aware of the program’s existence (as evidenced by high take-up rates), but much fewer understand the details of its structure (e.g., Ross Phillips 2001, Smeeding, Ross Phillips, and O’Connor 2002).
to report income exactly at the first kink of the EITC schedule, the point that maximizes net tax refunds (Saez 2010). We show that the degree of “sharp bunching” by self-employed individuals at the first kink varies substantially across ZIP codes in the U.S. For example, 7.4% of EITC claimants in Chicago, IL are self-employed and report total earnings exactly at the refund-maximizing level, compared with 0.6% in Rapid City, SD. Bunching spreads across the U.S. and increases sharply over time: the degree of bunching is almost 3 times larger in 2009 than in 1996.

The key assumption needed to use sharp bunching as a proxy for knowledge about the EITC schedule is that individuals in low-bunching neighborhoods believe that the EITC has no impact on their marginal tax rates. We present evidence supporting this assumption in two steps. First, we show that the spatial heterogeneity in bunching is driven primarily by differences in knowledge about the first kink of the EITC schedule. We find that those who move from low-bunching to high-bunching neighborhoods are much more likely to report incomes that yield larger EITC refunds after they move. In contrast, those who move from high-bunching to low-bunching neighborhoods continue to obtain larger EITC refunds even after they move. The persistent effects of high-bunching (but not low-bunching) neighborhoods after individuals move strongly suggests that neighborhoods affect bunching via learning, as other factors would be unlikely to have such asymmetric impacts. Moreover, we find that bunching is highly correlated with predictors of information diffusion, such as the density of EITC recipients, the availability of professional tax preparers, and the frequency of Google searches for phrases including the word “tax” (e.g., “tax refund” or “Earned Income Tax Credit”) in a neighborhood. In contrast, variation in local tax compliance rates or state policies explain little of the variation in bunching. Second, we show that individuals in low-bunching areas are unaware not just about the refund-maximizing kink but about the EITC schedule more broadly. In particular, when individuals become eligible for a much larger EITC refund after having their first child, the distribution of their reported self employment income remains virtually unchanged in low-bunching areas. This result establishes that individuals in low-bunching areas behave as if the EITC does not affect their marginal incentives, as required for our approach.

In the second half of the paper, we use neighborhoods with low levels of sharp bunching among the self-employed (i.e., low-knowledge neighborhoods) as counterfactuals to identify the causal

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3 In Chetty et al. (2012), we use data from tax audits to show that this sharp bunching among the self-employed is driven primarily by non-compliance. For the analysis in this paper, it does not matter whether self-employed sharp bunching is due to manipulation of reported income or changes in real earnings.

4 If individuals in low-bunching areas have some knowledge of the EITC schedule, our approach underestimates the impact of the EITC on earnings behavior.
impact of the EITC on the wage earnings distribution. Unlike self-employment income, wage earnings are double reported by employers to the IRS on W-2 forms. The degree of misreporting of wage earnings is therefore minimal and changes in wage earnings primarily reflect changes in “real” choices rather than non-compliance (Andreoni et al. 1998, Slemrod 2007, Chetty et al. 2012). We find that the wage earnings distribution exhibits more mass around the refund-maximizing EITC plateau in neighborhoods with high self-employed sharp bunching. Wage-earners’ EITC refunds are on average 20% higher in neighborhoods in the highest sharp bunching decile relative to the lowest bunching decile. EITC refund amounts rise when wage-earners move to neighborhoods with high self-employment bunching. In contrast, moving from a high to a low bunching neighborhood does not decrease refund amounts, confirming that these effects on wage earnings are driven by learning.

The cross-neighborhood comparisons of wage earnings distributions do not definitively establish that the EITC has a causal effect on earnings because there could be other confounding differences across neighborhoods, such as differences in industrial structure or the supply of jobs. To account for omitted variable biases, we exploit the fact that individuals with no children are essentially ineligible for the EITC, thus creating a natural “control group” that can be used to account for any differences across neighborhoods that are not caused by the EITC. We implement this strategy using event studies of earnings around the birth of a first child, which effectively makes a household eligible for the EITC. The challenge in using child birth as an instrument for tax incentives is that it affects labor supply directly. We isolate the impacts of tax incentives by again using differences in knowledge about the EITC across neighborhoods to obtain counterfactuals. We find that wage earnings in low-bunching and high-bunching neighborhoods track each other closely in the years prior to child birth. However, when a first child is born, wage earnings distributions become much more concentrated around the EITC plateau in high-bunching ZIP codes, leading to larger EITC refunds in those areas. This result is robust to allowing for ZIP code level fixed effects, so that the impacts of the EITC on wage earnings are identified purely from within-area variation over time in the degree of knowledge about the schedule. Moreover, the birth of a third child – which has no impact on EITC refunds in the years we study – does not generate differential changes in earnings across areas. We conclude that unobservable differences across areas with different levels of sharp bunching are unlikely to drive our results and that the EITC has a causal impact on wage earnings.

We quantify the impacts of the EITC on average earnings behavior in the U.S. by comparing its impacts on the economy as a whole to its impacts in the lowest-bunching neighborhoods. We
find large differences between the program’s impacts on earnings in the phase-in and phase-out regions. Approximately 75% of the increase in EITC refunds due to behavioral responses comes from increases in earnings in the phase-in region of the schedule, with only 25% coming from reductions in earnings in the phase-out region. The increases in EITC refunds due to behavioral responses are commensurate to a phase-in earnings elasticity of 0.14 and phase-out earnings elasticity of 0.06 on average in the U.S. The phase-in and phase-out elasticities are 0.58 and 0.30 in the highest-knowledge areas.

One explanation for the larger responses in the phase-in is that structural labor supply elasticities are larger in the phase-in than the phase-out region, e.g. because individuals with very low incomes have higher elasticities than those holding a fixed, full time job. Another explanation is that, on average, individuals pay more attention to the phase-in and refund-maximizing plateau portions of the schedule than the phase-out region. This point illustrates a key feature of our research design: it identifies the impact of the EITC on earnings as it is currently perceived on average in the U.S. Changes in the structure of the program that make the phase-out incentives more salient – e.g., increasing the phase-out rate – or further diffusion of information could potentially amplify disincentive effects.

Overall, our results show that the EITC has raised net incomes at the low end of the income distribution significantly with limited work disincentive effects. The fraction of EITC-eligible wage-earners below the poverty line falls from 31.9% without the EITC to 22.0% by mechanically including EITC payments (holding earnings and reported incomes fixed). The fraction below the poverty line falls further to 21.0% once earnings responses to the EITC are taken into account. If knowledge about the EITC schedule were to increase to the level observed in the highest decile of bunching, the poverty rate would fall further to 19.6%.

Our results build on and relate to a large empirical literature on the impacts of the EITC on labor supply, surveyed by Hotz and Scholz (2003), Eissa and Hoynes (2006), and Meyer (2010). Several studies have documented clear evidence that the EITC increases labor force participation – the extensive margin response (e.g., Eissa and Liebman 1996, Meyer and Rosenbaum 2001, Eissa and Hoynes 2004, Grogger 2003, Hotz and Scholz 2006, Hotz et al. 2011, Gelber and Mitchell 2012). However, evidence on intensive margin responses is mixed and somewhat inconclusive (e.g., Meyer and Rosenbaum 1999, Bollinger et al. 2009, Rothstein 2010). The majority of the increase in EITC refunds we document here comes from individuals who were already working, providing the first non-parametric evidence that the EITC does in fact induce substantial intensive-margin responses.
Importantly, our findings do not necessarily imply that extensive margin responses are small. Our research design effectively compares areas with low vs. high levels of knowledge about the marginal incentives created by the EITC schedule. The knowledge that working can yield a large tax refund – which is all one needs to know to respond along the extensive margin – could be more widespread across all neighborhoods, perhaps because it has first-order returns (Chetty 2012). But responding along the intensive margin requires knowledge about the non-linear marginal incentives created by the EITC and has only second-order benefits, potentially leading to greater variation across areas and slower diffusion over time. Our results thus help explain why prior studies of the EITC have had less success in detecting intensive margin impacts than extensive margin impacts. More generally, the common wisdom that intensive margin responses to tax incentives are smaller than extensive margin responses may be an artifact of the research designs that have been used to study behavioral responses rather than a structural feature of the economy.

Our findings also contribute to the recent debate on whether EITC subsidies drive down wage rates in equilibrium, thereby limiting the extent to which the program raises net incomes (Rothstein 2010, Leigh 2010). Such general equilibrium effects are difficult to identify using traditional methods (e.g., difference-in-differences designs comparing women with and without children) because they affect both the treatment and control groups. Under the assumption that different geographic areas constitute separate labor markets, our comparisons of income distributions across neighborhoods incorporate general equilibrium changes in wage rates. Our results suggest that the EITC substantially increases earnings even when general equilibrium effects are taken into account.

Finally, our approach contributes to the recent literature on estimating the impacts of tax and transfer policies from bunching at kink points (e.g., Saez 2010, Chetty et al. 2011, Kleven and Waseem 2012) by identifying diffuse behavioral responses around kinks. Because wage-earners typically cannot control their earnings perfectly, the impact of the tax policies on the wage earnings distribution is diffuse and cannot be identified by studying the aggregate distribution. We leverage the ability to non-parametrically identify sharp bunching by self-employed tax filers through income manipulation to develop a counterfactual to identify wage-earners’ diffuse real earnings responses. This method allows us to identify the impact of tax policies on the full distribution of real earnings.

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5. 75% of eligible individuals claim the EITC (Plueger 2009), indicating that many individuals are aware of the program’s existence. This knowledge is likely due to IRS outreach efforts such as Taxpayer Assistance Centers (TAC) and Volunteer Income Tax Assistance (VITA). However, these programs focus on increasing take-up rather than disseminating information about the details of the non-linear marginal rate structure of the schedule.

6. Liebman (1998) and Hotz and Scholz (2003, p. 182) also suggest that a lack of information could explain why the EITC has small impacts on the intensive margin.
As we discuss in the conclusion, this approach could be used to identify the impacts of a variety of policies in environments with frictions.

The remainder of the paper is organized as follows. Section II presents a stylized model to formalize our research design. Section III provides background about the EITC and the dataset we use. Section IV documents the heterogeneity across neighborhoods in sharp bunching by the self-employed and shows that this heterogeneity is driven by differences in information. Section V presents our main results on the effects of the EITC on wage earnings. In Section VI, we use our estimates to calculate the impacts of the EITC on income inequality. Section VII concludes.

II Model and Research Design

In this section, we develop a stylized non-linear budget-set model of labor supply and tax compliance behavior to formalize our estimation strategy and identification assumptions. We make two simplifications in our baseline derivation. First, we assume that firms have constant-returns-to-scale technologies and pay workers a fixed pre-tax wage of \( w \). Second, we abstract from income effects in labor supply by assuming that workers have quasi-linear utility functions. We discuss how these assumptions affect our estimator after analyzing the baseline case.

Setup. Individuals, indexed by \( i \), make two choices: labor supply \( (l_i) \) and tax evasion \( (e_i) \). Let \( z_i = wl_i \) denote true earnings and \( \bar{z}_i = z_i - e_i \) denote reported taxable income. Workers face a two-bracket tax system that provides a tax credit for working. When \( \bar{z}_i < K \), workers face a marginal tax rate of \( \tau_1 < 0 \) (a subsidy for work). For earnings above \( K \), individuals pay a marginal tax rate of \( \tau_2 > 0 \) (a clawback of the subsidy). Let \( \tau = (\tau_1, \tau_2) \) denote the vector of marginal tax rates.\(^7\)

There are two types of workers: tax compliers and non-compliers. Non-compliers face zero cost of evasion and always choose to \( e_i \) to report \( \bar{z}_i = K \) and maximize their tax refunds (when they know the tax schedule, see below). Compliers face an infinite cost of altering their reported taxable income and hence always set \( e_i = 0 \).\(^8\)

Individuals have quasi-linear utility functions \( u(C_i, l_i, \alpha_i) = C_i - h(l_i, \alpha_i) \) over a numéraire consumption good \( C_i \) and labor supply \( l_i \). The parameter \( \alpha_i \) captures skill or preference heterogeneity across agents. Individuals cannot set \( l_i \) exactly at their utility-maximizing level because of frictions.

\(^7\)This simplifies the actual EITC schedule shown in Figure 1, which has a plateau region and two kinks. The case with one kink captures the key concepts underlying our research design.

\(^8\)For simplicity, we ignore other variable costs of evasion, such as the threat of an audit or fines. Allowing for such costs has no impact on the estimator we derive below.
and rigidities in job packages. Our empirical approach does not rely on a specific positive model of how such frictions affect labor supply choices. Because of these frictions, the empirical distribution of true earnings $F(z)$ exhibits diffuse excess mass around the refund-maximizing kink $K$ rather than sharp bunching at the kink $K$. As a result, traditional non-linear budget-set methods (e.g., Hausman 1981) and the bunching estimator proposed by Saez (2010) do not non-parametrically identify the impact of taxes on earnings behavior.

Our estimator exploits geographic heterogeneity for identification. To model such heterogeneity, we assume that there are $N$ cities of equal size in the economy, indexed by $c = 1, ..., N$. Workers cannot move to a different city. Cities differ in their residents’ knowledge about the tax credit for exogenous reasons. In city $c$, a fraction $\lambda_c$ of workers are aware of the marginal incentives $\tau_1$ and $\tau_2$ created by the tax credit. The remainder of the workers optimize as if $\tau_1 = 0$ and $\tau_2 = 0$ (denoted below by $\tau = 0$). Cities may also differ in the distribution of skills $\alpha_i$, denoted by a smooth cdf $G_c(\alpha_i)$, and in the fraction of non-compliers, $\theta_c$. Let $F_c(z|\tau)$ denote the empirical distribution of earnings in city $c$ with a tax system $\tau$.

**Identifying Tax Policy Impacts.** Our objective is to characterize the impact of the tax credit, as it is currently perceived by agents, on the aggregate earnings distribution:

$$\Delta F = F(z|\tau \neq 0) - F(z|\tau = 0).$$

The first term in this expression is the observed distribution of true earnings in the population given current knowledge of the tax credit and rates of non-compliance. The second term is the potential outcome without taxes, which is the unobserved counterfactual. Cities with no knowledge about the tax credit’s marginal incentives ($\lambda_c = 0$) can be used to identify this counterfactual distribution. In the absence of income effects, earnings decisions in these cities are identical to behavior with no taxes at all:

$$F_c(z|\tau \neq 0, \lambda_c = 0) = F_c(z|\tau = 0, \lambda_c = 0).$$

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9 In practice, differences in knowledge may arise from factors related to the structure of the city, such as population density, network structure, and the availability of tax preparation services.

10 To simplify notation, we assume that $\lambda_c$ is the same for compliers and non-compliers. If knowledge varies across the types, the estimator in (3) identifies the treatment effect of interest under the two assumptions below if $\lambda_c$ is interpreted as the average level of knowledge across all individuals in each city.

11 Recall from our model that non-compliers adjust solely evasion $e_i$ and hence their real earnings decisions $z_i$ are not affected by knowledge about marginal tax rates. Hence, a high fraction of non-compliers would lead to attenuates real earnings responses.

12 The traditional approach to identifying $F(z; \tau = 0|\lambda = \lambda_c)$ is to use behavior prior to a tax reform as a counterfactual. In practice, time series trends and the slow diffusion of information make it challenging to separate the causal impacts of the tax policy from confounding factors.
To use cities with $\lambda_c = 0$ as counterfactuals, we first need to measure the degree of knowledge of marginal incentives $\lambda_c$ in each city. We do so by taking advantage of the fact that we observe both reported income $\tilde{z}_i$ and true wage earnings $z_i$ in our data. The fraction of individuals in city $c$ who report taxable income $\tilde{z}_i$ exactly at the kink, which we denote by $\phi_c$, is equal to the product of local knowledge about the tax code and non-compliance rates:

$$\phi_c = \theta_c \lambda_c.$$  

Hence, the rate of sharp bunching at the kink $\phi_c$ is a noisy proxy for the degree of knowledge $\lambda_c$. To identify areas with $\lambda_c = 0$, we make the following assumption.

**Assumption 1 [Tax Knowledge].** Individuals in neighborhoods with no sharp bunching at the kink have no knowledge of the policy’s marginal incentives and perceive $\tau = 0$:

$$\phi_c = 0 \Rightarrow \lambda_c = 0.$$  

In our simple model, Assumption 1 is equivalent to requiring that $\theta_c > 0$ in all cities, i.e. that all cities have some non-compliers. In this case, a city with no sharp bunching at the kink must be a city in which no one knows about the tax incentives. More generally, the key assumption underlying our approach is that individuals in areas with no sharp bunching behave on average as if the credit induces no change in their marginal tax rates ($\tau = 0$). If some areas with $\phi_c = 0$ actually have knowledge about marginal incentives created by the tax code, our approach will understate the impact of tax policy on earnings behavior. The degree of this attenuation bias depends on the extent to which the variation in bunching $\phi_c$ across cities is driven by knowledge vs. compliance rates and other factors. While we are unable to directly test Assumption 1, we present evidence that knowledge is a key driver of variation in $\phi_c$ and that individuals in cities with $\phi_c$ close to 0 behave as if they face no change in taxes ($\tau = 0$) when they become eligible for the tax credit we study.

Under Assumption 1, the empirical distribution of earnings $F_c(z)$ in cities with no sharp bunching in reported taxable income at the kink $K$ reveals the distribution of earnings in those cities in the absence of taxes:

$$F_c(z|\tau \neq 0, \phi_c = 0) = F_c(z|\tau = 0, \phi_c = 0).$$

13Importantly, Assumption 1 does not require that $\phi_c$ is an accurate proxy for differences in knowledge across all cities; it only requires when $\phi_c$ is low, knowledge about marginal incentives created by the tax code is low. The second requirement is much weaker and perhaps more plausible.
Although (2) identifies the necessary counterfactual in cities with no knowledge of the tax code, estimating the treatment effect in (1) requires that we identify the mean earnings distribution across all cities in the absence of taxes, \( F(z|\tau = 0) = \frac{1}{N} \sum_{c=1}^{N} F_c(z|\tau = 0) \). This leads to the identification assumptions underlying our research design.

**Assumption 2a [Cross-Sectional Identification].** Individuals’ skills do not vary across cities with different levels of knowledge about the tax credit:

\[
G(\alpha_i|\lambda_c) = G(\alpha_i) \text{ for all } \lambda_c.
\]

This orthogonality condition guarantees that cities with low levels of sharp bunching at the kink have earnings distributions that are representative of other cities on average. Under this assumption, we obtain the following feasible non-parametric estimator for the treatment effect in (1):

\[
\widehat{\Delta F} = F(z|\tau) - F(z|\tau, \phi_c = 0).
\]

Intuitively, the impact of the tax credit on earnings can be identified by comparing the unconditional earnings distribution with the earnings distribution in cities with no sharp bunching (i.e., no knowledge) about the tax credit.\(^{14}\) Naturally, this identification strategy requires that the earnings distribution in cities with no bunching is representative of earnings distributions in other cities in the absence of taxes. We can relax this assumption by studying changes in behavior when an individual becomes eligible for the tax credit in panel data. Suppose we observe individuals making labor supply decisions for multiple years. Let \( t \) denote the year that an individual becomes eligible for the tax credit, e.g. by having a first child, which is the situation we will use in our empirical analysis. This panel design relies on a weaker “common trends” assumption for identification.

**Assumption 2b [Panel Identification].** Changes in skills when an individual becomes eligible for the credit do not vary across cities with different levels of knowledge about the tax credit:

\[
G_t(\alpha_i|\lambda_c) - G_{t-1}(\alpha_i|\lambda_c) = G_t(\alpha_i) - G_{t-1}(\alpha_i) \forall \lambda_c.
\]

Under Assumption 2b, we can identify \( \Delta F \) using a difference-in-differences estimator that compares earnings distributions across cities before vs. after individuals become eligible for the tax credit:

\[
\widehat{\Delta F}_{DD} = [F_t(z|\tau) - F_t(z|\tau, \phi_c = 0)] - [F_{t-1}(z|\tau) - F_{t-1}(z|\tau, \phi_c = 0)].
\]

\(^{14}\)In practice, there are no neighborhoods with exactly zero sharp bunching in the data. We therefore use the neighborhoods with very low levels of bunching as counterfactuals, which slightly attenuates our estimates.
The first term in (4) coincides with the cross-sectional estimator in (3). The second term nets out differences in earnings distributions across cities prior to eligibility for the credit. This estimator permits stable differences in skills across cities, but requires that skills do not trend differently across cities around the point at which individuals become eligible for the tax credit. We implement the estimator in (4) using the birth of a first child as an instrument for eligibility. Importantly, (4) permits a direct effect of child birth on labor supply as long as the effect does not differ across cities with different amounts of knowledge. Because of such direct effects, we cannot identify $\Delta F$ purely from changes in earnings behavior around the date of eligibility in the full population, again making comparisons across cities with different levels of knowledge essential for identification.\textsuperscript{15}

\textit{Income Effects and Changes in Wage Rates.} We now return to the implications of our two simplifying assumptions for our estimator for $\Delta F$. When firms do not have constant-returns-to-scale technologies, changes in labor supply induced by tax incentives will affect equilibrium wage rates. As a result, the impact of a tax policy on the equilibrium earnings distribution is a function of both labor supply changes and changes in wage rates. The cross-sectional estimator for $\Delta F$ in (3) incorporates any such general equilibrium (GE) effects because the earnings distributions in cities with more knowledge about the tax code incorporate both changes in $l_i$ and $w_i$. The difference-in-differences estimator in (4) nets out GE wage changes if individuals who are eligible and ineligible for the credit are pooled in the same market. By comparing the two estimates, one can in principle gauge the magnitude of GE effects provided that both Assumptions 2a and 2b hold.

When utility is not quasi-linear, taxes affect behavior through both price and income effects. Because individuals in all cities receive the tax credit we analyze irrespective of their perceptions, our cross-city comparisons essentially net out differences in behavior that arise purely from income effects. Hence, our estimator for $\Delta F$ approximately identifies compensated elasticities in a more general model without quasilinear utility.\textsuperscript{16}

\textsuperscript{15}In a more general model that permits heterogeneity in responses to taxation, (4) identifies the local average treatment effect of the EITC on wage earnings among households who have just had their first child.

\textsuperscript{16}The equivalence is not exact because price effects induce changes in earnings that in turn change the size of the EITC refund that individuals in high bunching areas receive. In practice, this change in the income transfer due to behavioral responses is negligible relative to the size of the EITC and hence generates only a second-order effect.
III Data and Institutional Background

III.A EITC Structure

The EITC is a refundable tax credit administered through the income tax system. In 2009, the most recent year for which statistics are available, 25.9 million tax filers received a total of $57.7 billion in EITC payments (Internal Revenue Service 2011a, Table 2.5). Eligibility for the EITC depends on total earnings – wage earnings plus self-employment income – and the number of qualifying children. Qualifying dependents for EITC purposes are relatives who are under age 19 (24 for full-time students) or permanently disabled, and reside with the tax filer for at least half the year.\footnote{Only one tax filer can claim an eligible child; for example, in the case of non-married parents, only one parent can claim the child.} Eligibility for the EITC is also limited to tax filers who are US citizens or permanent residents with a valid Social Security Number (SSN).

Figure 1a displays the EITC amount on the right y-axis as a function of earnings for single filers with one or two or more qualifying dependents throughout our period, expressed in real 2010 dollars. EITC refund amounts first increase linearly with earnings, then plateau over a short income range, and are then reduced linearly and eventually phased out completely. In the phase-in region, the subsidy rate is 34 percent for taxpayers with one child and 40 percent for taxpayers with two or more children. In the plateau (or peak) region, the EITC is constant and equal to a maximum value of $3,050 and $5,036 for filers with 1 and 2+ children, respectively. In the phase-out region, the EITC amount decreases at a rate of 15.98\% for filers with 1 child, and 21.06\% for those with 2+ children. The EITC is entirely phased-out at earnings equal to $35,535 and $40,363 for single filers with 1 and 2+ children, respectively. Tax filers with no dependents are eligible for a small EITC refund, with a maximum credit of $457 and a subsidy and clawback rate of 7.65\%. As both the rates and levels are an order of magnitude smaller than for households with children, we exclude filers with no children from our analysis of the credit’s treatment effects and use the term “EITC recipients” to refer exclusively to EITC recipients with at least one qualifying child. See IRS Publication 596 (Internal Revenue Service 2011b) for complete details on program eligibility and rules.

Aside from inflation indexing, the structure of the EITC has remained stable since 1996 after the large EITC expansion from 1994 to 1996, with two small exceptions. First, for those who are married and filing jointly, the plateau and phase-out regions of the EITC were extended by $1,000 in 2002-04, $2,000 in 2005-07, $3,000 in 2008, and $5,000 in 2009-11 (and indexed for inflation after
Second, a slightly larger EITC was introduced for families with three or more children in 2009. For these households, the phase-in rate is 45% (instead of 40%) with a maximum EITC of $5,666 as of 2010. The location of the plateau remains the same as for those with two children for this group. The stability of the EITC schedule could facilitate the diffusion of information about the program’s parameters that we document below.

Note that other aspects of the tax code such as the Child Tax Credit and income taxes also affect individuals’ budget sets. Our estimates incorporate any differences across neighborhoods in knowledge about these other aspects of the tax code as well. However, marginal tax rates in the income range we study are primarily determined by the EITC; the child tax credit and federal income tax rates have relatively small effects on incentives, as shown in Appendix Figure 1.\footnote{The Child Tax Credit is only partially refundable and therefore for most of our sample period has no impact on the budget set in the phase-in region. It is quantitatively small relative to the EITC; the maximum Child Tax Credit per child is $500 before 2001 and $1,000 starting in 2001. Federal income taxes and state income taxes typically affect the budget set starting in the phase-out region because of exemptions and deductions.} Moreover, most of the earnings response we find comes from the phase-in region of the EITC schedule, where marginal incentives are essentially unaffected by other aspects of the tax code. We therefore interpret our estimates as the impacts of the EITC on earnings behavior.

\section*{III.B Sample and Variable Definitions}

We use selected data from the universe of United States federal income tax returns spanning 1996-2009. Because the data start in 1996, we cannot analyze the large 1994 EITC expansion that has been used in previous work. We draw information from income tax returns (i.e., individual income tax form 1040 and its supplementary schedules) and third-party reports on wage earnings (W-2 forms). This section describes the main variables used in our empirical analysis – income, number of children, and ZIP code of residence – and the construction of our analysis samples. In what follows, the year always refers to the tax year (i.e., the calendar year in which the income is earned). In most cases, tax returns for tax year $t$ are filed from late January to mid-April of calendar year $t+1$. As mentioned above, we express all monetary variables in 2010 dollars, adjusting for inflation using the official IRS inflation parameters used to index the tax system. Therefore, with the exception of the two legislated reforms described above, the EITC schedule remains unchanged in real terms across years.

\textit{Variable Definitions for Tax Filers.} We use two earnings concepts in our analysis, both of which are defined at the household (tax return) level because the EITC is based on household income. The first, total earnings, is the total amount of earnings used to calculate the EITC. This
is essentially the sum of wage earnings and net self-employment earnings reported on the 1040 tax returns.\textsuperscript{19} Total earnings correspond to reported income \( \hat{z}_i \) in our model.

The second earnings concept, wage earnings, is the sum of wage earnings reported on all W-2 forms filed by employers on the primary and secondary filer’s behalf. Data from W-2 forms are available only from 1999 onward. For this reason, we focus primarily on the period from 1999-2009 when analyzing wage earnings impacts. However, our event studies of earnings around child birth track individuals over several years and require measures of wage earnings prior to 1999. In these cases, we define wage earnings as total wage earnings reported on the 1040 tax return form for 1996-1998.\textsuperscript{20} We trim all income measures at -$20K and $50K to focus attention on the relevant range for the EITC.

For married individuals filing jointly, we assign both individuals in the couple the household-level total earnings and wage earnings because the EITC is based on household income. However, we structure our analysis based on an individual-level panel to account for potential changes in marital status. Because we define earnings at the family level, changes in marital status can affect an individual’s earnings even if his or her own earnings do not change.\textsuperscript{21}

We define the number of children as the number of children claimed for EITC purposes. The EITC children variable is capped at 2 from 1996-2007 and 3 in 2009. For individuals who report the maximum number of EITC children, we define the number of children as the maximum of EITC children and the number of dependent children claimed on the tax return. If the number of children claimed for EITC purposes is missing because the tax return does not claim the EITC (e.g., because earnings are above the eligibility cutoff), we define the number of children as the number of dependent children.\textsuperscript{22}

Finally, we define ZIP code as the ZIP code from which the individual filed his year \( t \) tax return. If an individual did not file in a given tax year, then we use the ZIP code reported as the home

\textsuperscript{19}More precisely, total earnings is the sum of the wage earnings line on the 1040 plus the Schedule C net income line on the 1040 form minus 1/2 of the self-employment tax on the 1040 adjustments to gross income. This adjustment is made in the tax code to align the tax treatment of wage earnings and self-employment earnings for Social Security and Medicare taxes. These taxes are split between employers and employees for wage earners, and wage earnings are reported net of the employer portion of the tax.

\textsuperscript{20}Total wage earnings reported on the tax return also include some minor forms of wage earnings not reported on W-2 forms, such as tips. The W-2 earnings measure is preferable because individuals could misreport wage income that is not third party reported on W-2 forms. None of our results are sensitive to the exclusion of pre-1999 data because we only use these data to assess pre-period trends, as discussed in greater detail below.

\textsuperscript{21}We have checked that our results are not driven by marriage effects by re-doing the analysis using solely individual earnings, instead of family earnings.

\textsuperscript{22}The requirements for EITC-eligible children vs. dependent children are not identical, but the difference is minor in practice. According to our calculations from the 2005 Statistics of Income Public Use Microdata File, less than 10% of EITC filers report different numbers for dependent children and EITC children.
address on the W-2 with the largest earnings reported for that individual in that year.

We do not observe total earnings or number of children for individuals who do not file tax returns, and we do not observe ZIP code for individuals who neither file nor earn wages reported on a W-2. These missing data problems can potentially create selection bias, which we address in our child birth sample below.

Core Sample. Our analysis sample includes individuals who meet all three of the following conditions simultaneously in at least one year between 1996 and 2009: (1) file a tax return as a primary or secondary filer (in the case of married joint filers), (2) have total earnings below $50,000 (in 2010 dollars), and (3) claim at least one child. We impose these restrictions to limit the sample to individuals who are likely to be EITC-eligible at least once between 1996 and 2009. We also remove observations with ITINs from the sample. We define the total earnings and wage earnings of person-year observations with no reported earnings activity as zero. These include individuals who do not file a tax return and have no W-2 wage earnings, individuals who die within the sample period, and individuals who leave the United States. This procedure yields a balanced panel with no attrition, i.e. every individual has exactly fourteen years of data. We refer to the resulting sample as our core analysis sample. The core sample contains 77.6 million unique individuals and 1.09 billion person-year observations on earnings. Our empirical analysis consists of three different research designs, each of which uses a different subsample of this core sample.

Cross-Sectional Analysis Sample. Our first research design compares earnings distributions for EITC claimants across cities in repeated cross-sections. For this cross-sectional analysis, we limit the core sample to person-years in which the individual files a tax return, reports one or more children, has total earnings in the EITC-eligible range, and is the primary filer. By including only primary filers, we eliminate duplicate observations for married joint filers and obtain distributions of earnings that are weighted at the tax return (family) level, which is the relevant weighting for tax policy and revenue analysis. Note that this cross-sectional sample excludes non-filers and thus could in principle yield biased results if EITC take-up rates vary endogenously across cities. We cannot resolve this problem in cross-sections because we do not observe non-filers’ number of children. We therefore address this issue using panel data in our third research design below.

Movers Sample. Our second research design tracks individuals as they move across neighborhoods. To construct the sample for this analysis, we first limit the core sample to person-years in

\[23\] The IRS issues ITINs to individuals who are not eligible for a Social Security Number (and are thus ineligible for the EITC). These individuals include undocumented aliens and temporary US residents, and account for 2.6% of our core sample.
which an individual files a tax return, claims one or more children, and has income in the EITC-eligible range. We then further restrict the data to individuals who move across 3-digit ZIP codes (ZIP-3s) in some year between 2000 and 2005. We impose these restrictions to ensure that we have at least four years of data on earnings before and after the move. In addition, this restriction also guarantees that we have W-2 (employer reported) wage earnings data for at least one year before the move. We define a move as a change in ZIP-3 between two consecutive years for which address information is available. When individuals move more than once, we include only the first move (as well as 4 years on either side, regardless of the timing of the second move). Note that we observe address at the time of tax filing, which in the EITC population is typically February of year \( t + 1 \) for year \( t \) incomes. A change in address for tax year \( t \) therefore implies that the move most likely took place between February of year \( t \) and February of year \( t + 1 \). A small fraction of the moves classified as occurring in year \( t \) thus do not take place till shortly after the end of that year. Importantly, none of the moves classified as occurring in year \( t \) occur prior to year \( t \) with this definition, ensuring that any misclassification errors do not affect pre-move distribution and only attenuate post-move impacts.

**Child Birth Sample.** Our third research design tracks individuals around the year in which they have a child, which can trigger eligibility for a larger EITC. We observe dates of birth as recorded by the Social Security administration. As in the movers sample, we restrict attention to births between 2000 and 2005 to ensure that we have at least 4 years of earnings data before and after child birth and at least one year of pre-birth W-2 earnings data. Next, we define the parents of the child as all the primary and secondary filers that claim the child either as a dependent or for EITC purposes within 5 years of the child’s birth. If the child is claimed by multiple individuals (e.g., a mother and father filing jointly), we define both individuals as new parents and track both parents over time. We then limit the core sample to the set of all such new parents, including all observations regardless of whether the individuals files a tax return in a given year.

In our child birth sample, we impute non-filers’ earnings, addresses, and number of children as follows. Because marital status is only observed on income tax forms, we cannot identify spouses for non-filers. We assume that non-filers are single and define both their total earnings and wage

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24 We include both primary and secondary filers to avoid excluding a subset of observations for individuals who change marital status within our sample. We account for repeated observations for married joint filers by clustering standard errors as described below.

25 See Section IV.B below for a detailed description of ZIP-3s.

26 As in the movers sample, we include all individuals (both primary and secondary filers) rather than families here to avoid dropping observations when marital status changes.
earnings as the total income reported on W-2 forms. We code total earnings as zero for non-filers who have no W-2's. Throughout the sample, we assign individuals the ZIP code in which they lived during the year in which the child was born. For non-filers, we impute the ZIP code as the ZIP code to which a W-2 form was mailed in the year of child birth if available. 11.6% of households neither filed a tax return nor had W-2 information in the year their child was born; for this group we use the first available ZIP code after the child was born. Finally, we impute the number of children for non-filers as the minimum of the children claimed in the closest preceding and subsequent years in which the individual filed (not including the child who was born in year 0).

With these imputations for non-filers, the child birth sample includes all years for every individual who (1) has a child born between 2000 and 2005 according to Social Security records and (2) claims that child on a tax return at some point after his birth. Treating the decision to have a child as exogenous, the only selection into this child birth sample comes from the potentially endogenous decision to claim a child as a dependent. We account for potential selection bias through this channel using data prior to child birth as described below.

Descriptive Statistics. Table I presents summary statistics for our cross-sectional analysis sample using data from 1999-2009, the years in which we have W-2 earnings information. Mean total earnings are $20,091. The majority of this income comes from wage earnings: mean wage earnings as reported on W-2’s are $18,308. 19.6% of tax filers report non-zero self-employment income and the mean (unconditional) self-employment income in the sample is $1,770. Individuals in this population receive substantial EITC refunds, with a mean of $2,543. Nearly 70% of the tax returns are filed by a professional preparer. The population of EITC eligible individuals consists primarily of relatively young single women with children.

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27 Excluding elderly households who receive Social Security Income, over 90% of non-filers are single (Cilke 1998, Table 1, p. 15). Because our sample requires having a child birth at some point within the sample, it contains very few elderly households. Self-employment earnings are not observed if the individual does not file and are assumed to be zero.

28 This procedure codes total earnings and wage earnings as 0 for non-filers prior to 1999, when W-2 data are unavailable. Most non-filers have very low W-2 earnings when data are available, so this imputation is likely to be accurate for most cases. As noted above, none of our results are sensitive to the exclusion of pre-1999 data.

29 For individuals with multiple W-2 forms, we use the W-2 with the largest amount of earnings and non-missing address information.

30 While we cannot be certain about the number of dependents living with an individual in years she does not file, it is more likely that the number of children is the minimum of the lead and lag as children are sometimes exchanged (for tax reporting purposes) across parents. Individuals who do not file are therefore likely to have fewer children.

31 The empirical literature on the EITC has found no evidence that the EITC affects marriage and fertility decisions (Hotz and Scholz 2003, p. 184).
IV Neighborhood Variation in Bunching and EITC Knowledge

In this section, we develop a proxy for local knowledge about the EITC in four steps. First, we document sharp bunching at the first kink of the EITC schedule by self-employed individuals in the aggregate income distribution. Second, we show that the degree of sharp bunching varies substantially across neighborhoods in the U.S. Third, we present evidence that this spatial variation is driven by differences in knowledge about the refund-maximizing kink of the EITC schedule rather than other factors such as local tax compliance rates. Finally, we show that individuals in low-bunching areas are unaware not only of the refund-maximizing kink but behave as if the EITC does not affect their marginal tax rates at all income levels. Together, the results in this section establish that self-employed sharp bunching is a proxy for local knowledge that satisfies Assumption 1 above.

IV.A Aggregate Distributions: Self-Employed vs. Wage Earners

Figure 1a plots the distribution of total earnings for EITC claimants in 2008 using our cross-sectional analysis sample. The distribution is a histogram with $1,000 bins centered around the first kink of the EITC schedule. We plot separate distributions for EITC filers with one and two or more children, as these individuals face different EITC schedules, shown by the solid lines in the figures. Both distributions exhibit sharp bunching at the first kink point of the corresponding EITC schedule, the point that maximizes tax refunds net of other income tax liabilities (such as payroll taxes). This sharp bunching shows that the EITC induces significant changes in reported income, confirming Saez’s (2010) findings using public use samples.

Figure 1b replicates Figure 1a restricting the sample to wage-earners, defined as households who report zero self-employment income in a given year. In this figure, there is no sharp bunching at the EITC kinks, implying that all the sharp bunching in Figure 1a is due to the self-employed. However, one cannot determine from Figure 1b whether the EITC has an impact on the wage earnings distribution. The impact for wage-earners is likely to be much more diffuse because they cannot control their earnings perfectly due to frictions (Chetty et al. 2011). One therefore needs counterfactuals for the distributions in Figure 1b to identify the impacts of the EITC on wage earnings. We show below that the wage earnings distributions in Figure 1b are in fact reshaped by the EITC, but one would have no way of detecting such responses by studying only the aggregate

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32 These and subsequent figures include both single and married individuals. Married individuals face an EITC schedule with a slightly longer plateau region but the same first kink point. The EITC schedules shown in Figure 1 are for single individuals.
distribution.\footnote{There is no need for a counterfactual to estimate sharp bunching among the self-employed because there is no reason to expect point masses in the income distribution at the kinks of the tax schedule except for the impact of the tax system itself. By leveraging our ability to non-parametrically identify sharp bunching without a counterfactual, we develop counterfactuals to identify the diffuse response of wage earners.}

We develop counterfactuals for the wage earnings distribution using the research design described in Section II. To implement the approach empirically, we interpret the sharp bunching among the self-employed as a measure of manipulation in total earnings ($\tilde{z}_i$ in the model) and wage earnings reported on W-2’s as true earnings ($z_i$). Because wage earnings are double reported by employers to the IRS through W-2 forms, individuals have little scope to misreport wage earnings.\footnote{Any discrepancy between an individual tax return self-report and the employer W2 information return report is automatically detected by the IRS and can trigger an audit. Misreporting wage earnings therefore requires collusion between employers and employees, which is likely to be difficult especially in large firms. We show below that our results hold in the subsample of wage earners working at firms with more than 100 employees.}

In contrast, there is no systematic third-party reporting system for self-employment income and the expenses and profits of small businesses are difficult to verify, making it much easier to misreport self-employment income. Random audits reveal substantial misreporting of income among self-employed individuals, whereas compliance rates for wage earnings exceed 98% (Internal Revenue Service 1996).\footnote{For instance, the rate of income under-reporting for small business suppliers was over 80 percent in 1992 (Internal Revenue Service 1996, Table 3, page 8).}

In a companion paper (Chetty et al. 2012), we replicate this finding within the EITC population using audit data from the 2001 National Research Program. We find that the majority of the sharp bunching at the first kink of the EITC schedule among the self-employed is due to non-compliance, as the degree of sharp bunching in the post-audit total earnings distribution falls to $1/3$ of the original level. In contrast, misreporting among wage-earners is negligible even around the refund-maximizing region of the schedule, supporting the view that wage earnings represent true earnings $z_i$.

In the remainder of this section, we focus on total earnings ($\tilde{z}_i$) and analyze variation across neighborhoods in the degree of self-employed sharp bunching at the first kink of the EITC schedule.

\section*{IV.B Spatial Heterogeneity in Sharp Bunching}

We analyze spatial heterogeneity at the level of three-digit ZIP codes, which we refer to as ZIP-3s.\footnote{Standard (5 digit) ZIP codes are typically too small to obtain precise estimates of income distributions. Common measures of broader geographical areas such as counties or MSA’s are more cumbersome to construct in the tax data or do not cover all areas. There are 899 ZIP-3s in use in the continental United States, shown by the boundaries in Figure 2. ZIP-3 are typically (but not always) contiguous and are smaller in dense areas. For example, in Boston, the 021 ZIP-3 covers roughly the same area as the metro area’s subway system.} We define the degree of self-employed sharp bunching in a ZIP-3 as the percentage of EITC claimants who report total earnings at the first EITC kink and have non-zero self employment
income. More precisely, for ZIP-3 \( c \) in year \( t \), let \( \text{num}_{ct} \) denote the number of primary tax filers who claim the EITC with children, report non-zero self employment income, and report total earnings within \$500 of the first kink. Let \( \text{denom}_{ct} \) denote the total number of primary tax filers with children in ZIP-3 \( c \) in year \( t \) in our cross-sectional analysis sample. We define self-employed sharp bunching \( b_{ct} \) as \( \frac{\text{num}_{ct}}{\text{denom}_{ct}} \). Note that this definition incorporates both intensive and extensive margin changes in reporting self-employment income. Thus, part of the variation in bunching across areas is driven by differences in rates of reporting self-employment income, some of which is endogenous to knowledge about the EITC as we show below.\(^{37}\)

Figure 2 illustrates the spatial variation in \( b_{ct} \) in 2008 across the 899 ZIP-3s in the United States. To construct this figure, we divide the raw individual-level cross-sectional data in 2008 into 10 deciles based on \( b_{ct} \), so that the deciles are population-weighted rather than ZIP-3 weighted.\(^{38}\) Higher deciles are represented with darker shades on the map. The mean (population weighted) level of \( b_{ct} \) in the U.S. in 2008 is 2.4%. To gauge magnitudes, recall that the mean self-employment rate in our sample is approximately 20%; hence, if 10% of self-employed EITC claimants report total earnings at the kink, we would observe \( b_{ct} = 2\% \).

There is substantial dispersion in self-employed sharp bunching across neighborhoods in the U.S.\(^{39}\) For example, bunching rates are less than 0.5% in most parts of North and South Dakota, but are over 5% in some parts of Texas and Florida. While some of the variation in bunching occurs at a broad regional level – for example, bunching is greater in the Southern states – there is considerable variation even within nearby geographical areas. For example, the Rio Grande Valley in Southern Texas has self-employed sharp bunching of \( b_{ct} = 6.6\% \); in contrast, Corpus Christi, TX, which is 150 miles away, has bunching of \( b_{ct} = 2.3\% \). Moreover, there are no obvious discontinuities at state borders, suggesting that differences in state policies such as state EITC's are unlikely to explain the heterogeneity, a result that we verify formally below.

Appendix Figure 3 replicates Figure 2 for earlier years, beginning in 1996, the first year of our dataset and the year in which the EITC was expanded to its current form. To illustrate variation

\(^{37}\)We have assessed the robustness of our results to several alternative measures of sharp bunching, including (a) defining the denominator using only self-employed individuals rather than the full population to eliminate variation arising from differences in self-employment rates; (b) defining narrower and wider bands than \$500 around the kink; and (c) calculating excess mass relative to a smooth polynomial fit as in Chetty et al. (2011). Because self-employed bunching is so sharp (as shown in Figure 1), our results are essentially unchanged with these alternative definitions. As an illustration, we replicate our main results using the definition in (a) in Appendix Figure 2.

\(^{38}\)Visually, most of the country appears to be in the lower bunching deciles because bunching rates are much higher in dense neighborhoods, as we show below.

\(^{39}\)Given the sample sizes – which are on average 23,000 returns per ZIP-3 – bunching rates are essentially estimated without error and we therefore ignore the impact of imprecision in our estimates of \( b_{ct} \).
over time, we divide the observations into deciles after pooling all years of the sample, so that the
decile cut points remain fixed across years. Initially, sharp bunching was highly prevalent in a
few areas with a high density of EITC filers, such as southern Texas, New York City, and Miami.
Bunching has since spread throughout much of the United States and continues to rise over time.

Figure 3 plots the distribution of total earnings for individuals living in the lowest and highest
bunching deciles in the pooled sample from 1996-2009. This figure includes individuals with both 1
and 2+ children by plotting total earnings minus the first kink point of the relevant EITC schedule,
so that 0 denotes the refund-maximizing point. In the top decile, more than 8% of tax filers report
total earnings exactly at the refund-maximizing kink. In contrast, there is virtually no bunching
at this point in neighborhoods in the bottom decile, suggesting that these neighborhoods could
provide a good counterfactual for behavior in the absence of the EITC if the lack of sharp bunching
is due to a lack of knowledge about the EITC schedule.

IV.C Is the Variation in Bunching Driven by Knowledge?

We evaluate whether the differences in self-employed sharp bunching across ZIP-3s are driven by
differences in knowledge about the refund-maximizing kink of the schedule using two tests. First,
we analyze individuals who move across ZIP-3s and test for learning. Second, we correlate bunching
rates with proxies for the rate of information diffusion and competing explanatory factors such as
tax compliance rates.

Movers. Our hypothesis that the variation in bunching is driven by differences in knowledge
generates two testable predictions about the behavior of movers. The first is learning: individuals
who move to a higher bunching area should learn from their neighbors and begin to respond to
the EITC themselves. The second is memory: individuals who leave high bunching areas should
continue to respond to the EITC even after they move to a lower bunching area. This asymmetric
impact of prior and current neighborhoods distinguishes knowledge from other explanations for
heterogeneity in bunching. For instance, variation in preferences or tax compliance rates across
areas do not directly predict that an individual’s previous neighborhood should have an asymmetric
impact on current behavior.

We implement these two tests using the movers sample defined in Section III, which includes all
individuals in our core sample who move across ZIP-3s at some point between 2000 and 2005. This
sample includes 21.9 million unique individuals and 54 million observations spanning 1996-2009.
We define the degree of bunching for prior residents of ZIP-3 c in year t as the sharp bunching
rate for individuals in the cross-sectional analysis sample living in ZIP-3 \( c \) in year \( t - 1 \). We then divide the ZIP-3-by-year cells into deciles of prior residents’ bunching rates by splitting the individual-level observations in the movers sample into ten equal-sized groups. Note that with this definition, ZIP-3s may change deciles over time if their bunching rates rise or fall.

Figure 4a plots an event study of bunching for movers around the year in which they move. To construct this figure, we first define the year of the move as the first year a tax return was filed from the new ZIP-3. We then compute event time as the calendar year minus the year of the move, so that event year 0 is the first year the individual lives in the new ZIP-3. For illustrative purposes, we focus on individuals who live in a ZIP-3 in the fifth decile of the overall bunching distribution in the year prior to the move. We then divide this sample into three groups based on where they move in year 0: the first, fifth, and tenth bunching deciles. We calculate the sharp bunching rate in each event year and subgroup as the fraction of EITC claimants in the relevant group who report total earnings at the first kink and have non-zero self employment income.

To obtain a point estimate of the effect of moving to decile 10, we regress an indicator for sharp bunching (i.e., reporting total earnings at the kink and non-zero self employment income) on an indicator for moving to decile 10, an indicator for event year 0, and the interaction of the two indicators. We estimate this regression restricting the sample to event years -1 and 0 and deciles 5 and 10, so that the coefficient on the interaction term \( \beta_{10} \) is a difference-in-differences estimate of the impact of moving to decile 10 relative to decile 5. We estimate treatment effects of moving to deciles 1 and 5 using analogous specifications, always using decile 5 as the control group. Standard errors, reported in Figure 4 in parentheses below the coefficient, are clustered at the destination ZIP-3-by-year-of-move level.

Bunching rates rise sharply by \( \beta_{10} = 1.9 \) percentage points for individuals who move to the highest bunching decile, rise by a statistically insignificant \( \beta_5 = 0.1 \) percentage points for those who stay in a fifth-decile area, and fall slightly (by \( \beta_1 = -0.4 \) percentage points) for those who move to the lowest bunching decile. Individuals rapidly adopt local behavior when moving to high bunching areas. The mean difference in self-employed sharp bunching rates for prior residents is 3.6 percentage points between the fifth and tenth deciles. Hence, movers to the top decile adopt \((2.0 - 1)/3.6 = 53\%\) of the difference in prior residents’ behavior within the first year of their move.

While sharp bunching is perhaps the clearest evidence of responding to the EITC, relatively few individuals report income exactly at the first kink. To evaluate whether individuals learn about the EITC schedule more broadly when they move, we plot mean EITC refunds by event
year in Figure 4b. Using a difference-in-differences specification analogous to that used in Figure 4a, we estimate that EITC refund amounts rise by $150 on average when individuals move to the highest bunching decile. The increase in sharp bunching at the first kink accounts for at most $1.9% \times 4,403 = 77$ of this increase.\textsuperscript{40} Hence, individuals report incomes that generate larger EITC refunds more broadly than just around the first kink when they move to areas with high levels of sharp bunching.

Figure 5 plots total earnings distributions in the years before and after the move for the three groups in Figure 4. This figure is constructed in the same way as Figure 3, pooling individuals with 1 and 2+ children and computing total earnings relative to the first kink of the relevant EITC schedule. Consistent with the results from the event studies, the fraction of individuals reporting total earnings exactly at the kink and around the refund-maximizing plateau increases significantly after the move for those moving to high bunching areas, consistent with learning.\textsuperscript{41} However, the distribution remains relatively stable for those moving to low bunching areas, consistent with memory.

To distinguish learning and memory more directly, we test for asymmetry in the impacts of increases vs. decreases in sharp bunching rates when individuals move. Figure 6 plots changes in mean EITC refunds from the year before the move (year -1) to the year after the move (year 0) vs. the change in local sharp bunching $\Delta b_{ct}$ that an individual experiences when he moves. Following standard practice in non-parametric regression kink designs, we bin the x-axis variable $\Delta b_{ct}$ into intervals of width 0.05% and plot the means of the change in EITC refund within each bin. If the variation in bunching is due to knowledge, there should be a kink in this relationship around 0: increases in $b_{ct}$ should raise refunds, but reductions in $b_{ct}$ should leave refunds unaffected. We test for the presence of such a kink by fitting separate linear control functions to the points on the left and right of the vertical line, with standard errors clustered by the bins of $\Delta b_{ct}$ (Card and Lee 2007). As predicted, the slope to the right of the kink is significant and positive: a 1 percentage point increase in sharp bunching at $b_{ct} = 0$ leads to a $60 increase in EITC refunds. In contrast, a 1 percentage point reduction in $b_{ct}$ leads to a statistically insignificant change in EITC refunds.

\textsuperscript{40}Roughly half of the individuals in the movers sample claim one child, while the other half claim two or more children. The weighted average of the maximum EITC refund across these groups is $4,043. $77 is a non-parametric upper bound on the impact of sharp-bunching on average EITC refunds; the actual effect is likely much smaller.

\textsuperscript{41}Individuals moving to decile 10 exhibit more bunching even prior to the move because our ZIP-3 measure of neighborhoods generates discrete jumps in neighborhood bunching at boundaries. Individuals who move to decile 10 are more likely to live in ZIP-3’s that are adjacent to decile 10 areas, and thus live in locally higher bunching areas even though their ZIP-3 is classified in decile 5 as a whole. This measurement error in neighborhood bunching works against the hypotheses we test.
of $6. The hypothesis that the two slopes are equal is rejected with \( p < 0.0001 \). The kink at zero constitutes non-parametric evidence of asymmetric responses to changes in bunching rates and therefore strongly indicates that at least part of the variation in \( b_{ct} \) is due to knowledge.\(^{42}\)

**Cross-Sectional Correlations.** To better understand the sources of variation in sharp bunching, we correlate \( b_{ct} \) with proxies for information, tax compliance, and other variables. While we cannot interpret these correlations as causal effects, the relative explanatory power of various factors sheds some light on why knowledge varies so much across areas.

Table II presents a set of OLS regressions of the rate of sharp bunching in each ZIP-3 on various correlates. Among a broad range of economic and demographic variables available from the 2000 decennial Census, the single strongest predictor of sharp bunching is the local density of EITC filers. In column 1 of Table II, we regress sharp bunching on density of EITC filers, defined as the number of EITC claimants with children (measured in 1000's) per square mile. We estimate the regression in a dataset that has one observation on sharp bunching per ZIP-3 in 2000 (the year of the Census) and weight by the number of EITC claimants in each ZIP-3. Increasing the density of EITC filers by 1,000 per square mile (a 1.6 SD increase) raises bunching rates by 1.93 percentage points (1.1 SD). The R-squared of the density variable by itself in a univariate regression (weighted by the number of filers in each ZIP-3) is 0.6. Intuitively, this regression shows that an isolated EITC recipient is less likely to learn about the schedule than one living amongst many other EITC eligible families.

The correlation between density and sharp bunching suggests that agglomeration facilitates the diffusion of knowledge in dense areas. Figure 7a documents this diffusion over time by plotting the average level of sharp bunching by year from 1996-2009. We split the sample into two groups: ZIP-3s with EITC filer density below vs. above the median in 1996. The degree of sharp bunching was relatively similar across these areas in 1996, the first year of the current EITC schedule. But rates of bunching rose much more rapidly in dense areas, presumably because information about the EITC schedule diffused more quickly in these areas.

Column 2 of Table II adds the following additional demographic controls to the specification in column 1: the percentage of the population that is foreign born, white, black, Hispanic, Asian, and other race. Bertrand et al. (2001) suggest that these demographic characteristics are related to the tightness of networks in low income populations. Consistent with this hypothesis, we find that

\(^{42}\)We show below that wage earnings exhibits similar asymmetric persistence, implying that individuals learn not just about non-compliance but also about the incentives that affect real work decisions.
these demographic characteristics explain a substantial share of the variation in sharp bunching beyond density, increasing the R-squared from 0.6 to 0.8.

Prior studies have also suggested that professional tax preparers may help disseminate information about the tax code (e.g., Maag 2005, Chetty and Saez 2012). To evaluate this hypothesis, in column 3 of Table II, we regress sharp bunching on the fraction of individuals who use a tax preparer in each ZIP-3 of our cross-sectional analysis sample in 2008. A 10 percentage point (1.5 SD) increase in the rate of local tax professional utilization is associated with a 0.986 percentage point (0.57 SD) increase in sharp bunching. Figure 7b plots the relationship between sharp bunching and the fraction of professionally prepared returns in the ZIP-3, dividing claimants into two groups based on whether they themselves used a tax preparer or not. This figure is a binned scatter plot, constructed by binning the x-axis into 20 equal-sized bins (quantiles) and plotting the means of $b_{ct}$ for each group in each bin. The figure shows that areas with high tax preparer penetration exhibit higher bunching among both groups. This result implies that tax professionals either serve simply as a seed for knowledge – informing their clients about the EITC who in turn spread the information to others – or that tax preparation firms locate endogenously in areas where EITC refunds are already high (Kopczuk and Pop-Eleches 2007).

Column 4 of Table II shows that sharp bunching is highly correlated with Google searches for information about taxes and tax refunds, another proxy for interest in and awareness about tax-related information. Following the techniques developed by Stephens-Davidowitz (2011), we measure the percentage of an area’s Google searches for any phrase that includes the word “tax” (such as “Earned Income Tax Credit” or “tax refund”) between 2004 and 2008. We divide this measure by its standard deviation to obtain a standardized measure. We regress sharp bunching on the Google search measure using the cross-sectional analysis sample in 2008, as internet usage rates were much lower in 2000 than 2008. A 1 SD increase in Google search intensity for “tax” in a ZIP-3 is associated with an 0.3 percentage point (0.17 SD) increase in sharp bunching. This association remains statistically significant when we add demographics, density, and professional tax preparation rates to the specification, as shown in column 5. Column 6 replicates column 5 with state fixed effects. EITC filer density, tax preparation services, and searches for information

43 Internet usage is substantial even amongst low SES populations: according to data from the CPS, 39% of individuals who did not graduate high school lived in a household with internet access in 2009 (U.S. Census Bureau 2012). We use the search term “tax” rather than more specific terms such as “EITC” because many individuals may not know the term “EITC” and because the Google search statistics are publicly available only for words that appear in a large number of searches.

44 Google search data are obtained at a media market level, which we map to ZIP-3’s using population-weighted averages.
about taxes remain highly predictive of within-state variation in sharp bunching.

Finally, we evaluate some competing explanations for the spatial variation in bunching. Column 7 shows that differences in state EITC top-up rates do not have a statistically significant impact on sharp bunching rates and explain relatively little of the variation in bunching. In column 8, we analyze whether differences in tax compliance rates \((\theta_c)\) across areas explain the variation in sharp bunching. We implement this analysis using data on random audits from the 2001 National Research Program as follows.\(^{45}\) First, we define a measure of non-compliance in each state as the fraction of non-EITC claimants who have adjustments of more than $1000 in their income due to NRP audits. We define non-compliance rates using individuals who do not receive the EITC to eliminate the mechanical correlation arising from the fact that individuals bunch at the kink primarily by misreporting total earnings. We then regress sharp bunching among EITC claimants in each state on the non-compliance rate, weighting by the number of individuals audited in each state to adjust for differences in sampling weights in the NRP. The correlation between sharp bunching and non-compliance rates is statistically insignificant, as shown in column 8. The non-compliance measure has an R-squared of less than 1\% by itself, suggesting that spatial variation in bunching is unlikely to be driven by heterogeneity in non-compliance.

In sum, the correlations indicate that a substantial fraction of the variation in sharp bunching across areas reflects differences in knowledge about the refund-maximizing kink of the EITC schedule that arise from structural features of local economies such as population density and demographic characteristics.

**IV.D Perceptions of the EITC in Low-Bunching Areas**

While the preceding evidence establishes that self-employed sharp bunching provides an informative (albeit noisy) proxy for local knowledge about the first kink of the EITC schedule, it does not directly establish that Assumption 1 holds. For instance, individuals who live in low-bunching areas may perceive the EITC to be a flat subsidy at a constant rate or a smoothly varying subsidy without kinks in the schedule. Such misperceptions would generate no bunching at the first kink but would imply that low-bunching areas do not provide a valid counterfactual for behavior in the absence of the EITC. We now present evidence that individuals in low-bunching areas actually appear to have no knowledge about the entire EITC schedule and behave as if \(\tau = 0\) on average.

\(^{45}\) State-level tabulations from NRP data were provided by the IRS Office of Research. Note that the NRP sampling frame was not explicitly designed to be representative at the state level, so the results here should be interpreted with caution.
when they become eligible for the credit.

We assess the beliefs of individuals in the lowest-bunching decile by examining changes in the distribution of reported self-employment income around the birth of a first child. As noted above, this event makes families eligible for a much larger EITC refund and sharply changes marginal incentives. We implement this analysis using our child birth sample, which includes approximately 15 million individuals from the core sample who have their first child between 2000 and 2005. We classify individuals into deciles of sharp bunching based on the level of $b_{ct}$, as measured from the cross-sectional sample, in the ZIP-3 and year in which he or she had a child.

Figure 8a plots the distribution of total earnings among self-employed individuals in the year before birth and the year of child birth. The distributions are scaled to integrate to the total fraction of individuals reporting self-employment income in each group, which varies across the groups as shown in Figure 8b below. The reported earnings distribution changes only slightly when individuals in the lowest-bunching decile have a child. In contrast, the distribution of total reported income exhibits substantial concentration both at and around the first kink for individuals in the top-bunching decile.\textsuperscript{46} The fact that the total earnings distribution remains virtually unchanged when individuals have a child in low-bunching areas implies that they perceive no changes in marginal incentives throughout the range of the EITC (rather than simply ignoring the first kink). For instance, if individuals in low-bunching areas perceived the EITC to be a constant subsidy, we would observe an upward shift in the total reported income distribution when individuals have a child and become eligible for the EITC.

Figure 8b conducts an analogous test on the extensive margin by plotting the fraction of individuals reporting self-employment income by event year around child birth, which is denoted by year 0. While there are clear trend breaks in the fraction reporting self-employment income around child birth in higher-bunching areas, there is little or no break around child birth in the lowest-bunching decile. Although we have no counterfactual for how self-employment income would have changed around child birth in low-bunching areas absent the EITC, we believe that the costs of manipulating reported self-employment income are unlikely to change sharply around child birth.\textsuperscript{47} Hence, the smooth trends in self-employment rates around child birth in the lowest-decile bunching areas

\textsuperscript{46}To simplify the figure, we only plot the distribution of earnings in the year before the birth for households in low-bunching neighborhoods. The pre-birth distribution in high bunching areas is similar to that in low-bunching areas; in particular, it does not exhibit any sharp bunching around the first kink of the EITC schedule.

\textsuperscript{47}Recall that the audit evidence reveals that changes in self-employment income are largely driven by non-compliance and hence reflect pure reporting effects. In contrast, child birth clearly has effects on real labor supply, making it crucial to have a counterfactual when using child birth as a quasi-experiment to identify wage earnings impacts as we do in Section V below.
provide further evidence that individuals in these areas do not perceive any change in incentives when they have a child.

Provided that individuals perceive \( \tau = 0 \) before they are eligible for the EITC, the results in Figure 8 imply that EITC-eligible individuals in low bunching areas perceive and behave as if \( \tau = 0 \) on average, as required by Assumption 1.\(^{48}\) We therefore proceed to use low-bunching neighborhoods as counterfactuals for behavior in the absence of the EITC. Note that we would ideally use areas with literally zero bunching as counterfactuals. In practice, there are very few areas with literally no sharp bunching, but the level of sharp bunching is very close to zero in the bottom decile, as shown in Figures 3 and 8a. We therefore use the lowest bunching decile as “no knowledge” areas to avoid extrapolations and maintain adequate precision to estimate counterfactual distributions. Our estimates slightly understate the causal impacts of the EITC because of this simplification.

V Effects of the EITC on Wage Earnings

In this section, we identify the impacts of the EITC on the distribution of real wage earnings using self-employed sharp bunching as a proxy for local knowledge about the EITC. We present estimates from two research designs. We first compare earnings distributions across neighborhoods in cross-sections. We then use child birth as a source of sharp changes in marginal incentives to obtain estimates from panel data that rely on weaker identification assumptions.

Throughout most of this section, we limit the sample to wage-earners (individuals who report zero self-employment income) and analyze wage earnings as reported by firms on W-2 forms. Note that restricting the sample based on self-employment income could in principle introduce selection bias, as the choice to report self-employment income is endogenous and depends upon knowledge about the EITC. In the last part of this section, we show that including all individuals and using W-2 wage earnings as the outcome yields similar but less precisely estimated results, implying that endogenous selection is not a significant source of bias in practice.

V.A Cross-Neighborhood Comparisons

We begin by comparing the distribution of wage earnings in ZIP-3s with low vs. high levels of sharp bunching. Identifying the causal impacts of the EITC using this research design requires

\(^{48}\) Individuals in the EITC income range who do not have children pay minimal taxes and receive minimal refunds; hence, it is most plausible that they perceive essentially zero marginal tax rates. These individuals may be aware of some aspects of the tax schedule, such as payroll or income taxes. In that case, our approach would identify the impact of the tax system including the EITC as it is perceived in the population on average relative to tax perceptions absent the EITC.
that areas with different levels of sharp bunching would have comparable earnings distributions in the absence of the EITC (Assumption 2a). In practice, there could be many differences across ZIP-3s with different levels of sharp bunching, as they differ in population density and various other characteristics as shown above. We nevertheless begin with cross-neighborhood comparisons because they provide a simple illustration of the main results and turn out to yield fairly similar estimates to those obtained below using our quasi-experimental design.

We compare earnings distributions across neighborhoods using our cross-sectional analysis sample, restricted to the years in which we have data on wage earnings from W-2’s (1999-2009). We pool the observations for wage-earners across all years in this dataset and divide the ZIP-3-by-year cells into ten deciles based on sharp bunching rates, weighting by the number of observations in each cell. Figure 9 plots the distribution of W-2 wage earnings for individuals in the lowest and highest deciles of $b_{ct}$. Panel A considers EITC recipients with one child, while Panel B considers those with two or more children. The vertical lines denote the beginning and end of the refund-maximizing EITC plateau. In both panels, there is an increased concentration of the wage earnings distribution around the refund-maximizing region of the EITC schedule in areas in the top decile of sharp bunching $b_{ct}$. Under Assumption 2a, we can interpret this result as evidence that the EITC induces individuals to choose earnings levels that yield larger EITC refunds in high-knowledge areas.\footnote{One may be concerned that the behavioral response occurs through differences in child claiming behavior across areas rather than earnings behavior. For instance, if divorced couples in high-knowledge areas are more likely to claim a child on the return that produces a larger EITC refund, we would see differences in earnings distributions as in Figure 9. We address this source of selection bias in the next subsection by exploiting exogenous information on the date of child birth.}

To characterize the excess mass more precisely, Figure 10 plots the difference between the earnings distributions for the highest and lowest deciles. For both the one child (Panel A) and 2+ child (Panel B) cases, the largest difference between the two densities occurs precisely in the refund-maximizing plateau region of the relevant schedule. As discussed above, audit studies reveal that W-2 earnings are rarely misreported, allowing us to interpret the differences in earnings distributions in Figure 10 as being driven by “real” labor supply choices rather than manipulation of reported income. The only potential source of misreporting on W-2’s is for firms to collude with workers to misreport W-2 earnings to the IRS, for instance by paying workers part of their earnings off the books. While such collusion may be feasible in small family firms, it is much less likely to occur in large firms given the complexity of sustaining collusion on a large scale (Kleven, Kreiner, and Saez 2009). To ensure that our results are not driven by collusive reporting effects, we repeat the analysis in Figure 10 for wage-earners working in firms with 100 or more employees.
Within this subgroup, the difference in earnings distributions between the highest and lowest sharp-bunching areas is very similar to that in the full sample. We therefore conclude that the wage earnings changes in high-bunching areas are not driven by reported earnings manipulation.\textsuperscript{50}

The analysis in Figures 9 and 10 considers only the first and tenth deciles of $b_{ct}$, the areas with the least and most knowledge about the EITC schedule. In Figure 11a, we extend the analysis to include all neighborhoods by plotting average EITC amounts for wage-earners vs. the level of sharp bunching $b_{ct}$ in their ZIP-3-by-year cell. The average EITC refund effectively measures the concentration of the earnings distribution around the refund-maximizing region of the schedule.\textsuperscript{51} Consistent with the earlier results, wage-earners in areas with high sharp bunching have earnings that produce significantly larger EITC refund amounts. A one percentage point increase in $b_{ct}$ raises the EITC refund by $15.9 on average. Wage-earners in the highest-bunching areas earn EITC refunds that are on average $122 (5.1\%)$ higher than those living in the lowest-bunching (near-zero knowledge) neighborhoods. Under Assumption 2a, this implies that behavioral responses to the EITC schedule raise EITC refund amounts by 5.1\% in the highest bunching decile.

\textit{Cross-Neighborhood Movers.} A natural approach to evaluate Assumption 2a and assess whether the level of knowledge in a neighborhood has a causal impact on earnings behavior is to again analyze changes in behavior for individuals who move across neighborhoods. Figure 11b plots changes in EITC refunds from the year before the move (event year -1) to the year after the move (event year 0) against the change in sharp bunching $b_{ct}$ from the old to the new neighborhood. This figure exactly replicates Figure 6, restricting the sample to wage earners. Note that Figure 11b can be interpreted a first-differenced version of Figure 11a, relating changes in EITC refunds to changes in local knowledge for movers using our movers analysis sample.

Figure 11b shows that wage-earners who move to higher $b_{ct}$ ZIP-3s change their earnings behavior so that their EITC refunds rise sharply. That is, increases in information in one’s neighborhood lead to earnings responses that raise EITC refund amounts. In contrast, for individuals who move to areas with lower levels of sharp bunching, the slope of the relationship has, if anything, the oppo-

\textsuperscript{50}One may be concerned that individuals in high-knowledge areas work in the formal sector up the point where they maximize their EITC refund and then work in informal jobs. Two pieces of evidence suggest that this is unlikely. First, our analysis of audit data (Chetty et al. 2012) shows that the likelihood of misreporting total earnings is no higher for individuals who report wage earnings in the plateau. Second, as we show below, most of the excess mass in the plateau comes from individuals raising W-2 earnings in the phase-in region in high-knowledge areas. The phase-in response cannot be driven by under-reporting of income from other jobs.

\textsuperscript{51}EITC refund amounts also vary with marital status and number of children. Although differences in these demographics across areas could in principle affect the estimate in Figure 11a, we find very similar results within each of these demographic groups.
We reject the null hypothesis that there is no kink in the slope of the control functions at 0 with \( p < 0.0001 \). This finding echoes the pattern of learning and memory documented above for the self-employed in Figure 6. The asymmetric persistence of past neighborhoods rules out a broad class of omitted variable biases that may arise from simple differences in characteristics across areas with different levels of sharp bunching. The finding that wage-earners making real decisions exhibit asymmetry also provides further evidence that the spatial heterogeneity in EITC response is due to knowledge about the schedule rather than tax compliance rates or other factors.\(^{53}\)

While these findings show that neighborhoods have a causal effect on individuals’ earnings behavior, they do not identify the extent to which individuals actively change their own behavior when exposed to more information about the EITC. Part of the increase in EITC refund amounts when individuals move to areas with higher \( b_{ct} \) in Figure 11b could in principle arise simply because individuals draw wage offers from a distribution that is more concentrated around the EITC plateau even if they do not actively reoptimize in response to the program incentives themselves.\(^{54}\) We now turn to a research design that allows us to isolate individuals’ responses to changes in incentives more precisely.

**V.B Impacts of Child Birth on Wage Earnings**

In this section, we implement a second research design to characterize the impact of the EITC on wage earnings behavior that does not rely purely on cross-neighborhood comparisons. Our strategy relies on the fact that individuals without children are eligible for only a very small EITC (see Section III) and therefore serve as a control group that can be used to net out differences across areas. We implement this strategy by studying changes in earnings around the birth of a first child. The first birth changes low-income families’ incentives to earn significantly and is thus a powerful instrument for tax incentives. The obvious challenge in using child birth as an

\(^{52}\) The only parameter that is non-parametrically identified in this figure is the kink at 0. The negative slope of the control function to the left of zero could be due to various factors that covary smoothly with the change in \( b_{ct} \). For instance, because individuals who experience large drops in \( b_{ct} \) come from high bunching areas, differences across areas in movers’ characteristics could generate differences in slopes. The identifying assumption underlying inference from the kink is that any such correlated factors have smooth impacts on the slopes.

\(^{53}\) For instance, one may be concerned that norms about tax compliance could have asymmetric persistence: once one observes someone else misreport earnings, it becomes an acceptable habit. The asymmetric persistence of wage earnings rules out such models and implies that individuals’ perception of incentives changes when they move to areas with high sharp bunching.

\(^{54}\) Another potential concern is reverse causality: areas with wage earnings distributions that have substantial mass around the plateau for exogenous reasons may end up having higher sharp bunching as individuals near the plateau learn how to earn larger refunds. It is difficult to explain the asymmetric pattern in Figure 11b purely with reverse causality, but it is possible that the magnitudes of the estimates obtained from cross-neighborhood comparisons are biased by such factors.
instrument for tax incentives is that it affects labor supply directly. We isolate the impacts of tax incentives by again using differences in knowledge across neighborhoods. In particular, we compare changes in earnings behavior around child birth for individuals living in areas with high levels of sharp bunching with those living in low-bunching areas. Low-bunching areas provide a counterfactual for how earnings behavior would change around child birth in the absence of the tax incentives.

We divide our child birth analysis sample into deciles based on sharp bunching in the individual’s ZIP-3 in the year of child birth, as described in Section IV.D. Figure 12 plots W-2 wage earnings distributions for wage-earners in the year before (Panel A) and the year of first child birth (Panel B). The distributions are reported for those living in deciles 1, 5, and 10 of the sharp bunching distribution when they have a child. In the year before child birth, the wage earnings distributions are virtually identical across areas with low vs. high levels of sharp bunching.\(^{55}\) However, an excess mass of wage-earners emerges around the plateau in high bunching areas immediately after birth, showing that individuals in these areas make an effort to obtain a larger EITC refund when making labor supply choices after child birth. Connecting this result to the cross-sectional correlations in Table II, Figure 12 essentially shows that individuals who live in areas with a high density of EITC filers have heard more about the credit by the time they have a child and therefore respond more strongly to its incentives.

The identification assumption underlying the research design in Figure 12 is that the direct impacts of child birth on earnings do not vary across neighborhoods with different levels of knowledge about the tax code (Assumption 2b). We assess the validity of this “common trends” assumption by examining trends prior to child birth using an event study design. Let year 0 denote the year in which the child is born (and hence the family becomes eligible for a larger EITC) and define event time relative to this year. Define an individual’s simulated EITC credit as the EITC an individual would receive given her wage earnings if she had one child and were single. This simulated EITC credit is a simple statistic for the concentration of the wage earnings distribution around the EITC plateau.\(^{56}\)

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\(^{55}\) In Table II, we showed that areas with higher sharp bunching have a higher density of EITC tax filers. This is not inconsistent with the result in Figure 12a. Figure 12a shows that the conditional earnings distributions among individuals just about to give birth are very similar across areas. However, the unconditional distributions differ across areas (e.g., because of differences in age and number of children). This is why we use an event study around child birth rather than comparisons of earnings distributions across all individuals with and without children for identification.

\(^{56}\) We use the simulated credit with fixed parameters in this analysis rather than the actual credit to separate changes in earnings from mechanical changes in credit amounts when individuals have children.
Figure 13 plots the simulated EITC by event year for wage earners with incomes in the EITC-eligible range for exactly the same three groups as in Figure 12. For scaling purposes, we normalize the level of each series at the mean simulated credit in $t = -4$ by subtracting the decile-specific mean in $t = -4$ and adding back the mean simulated EITC across the three deciles in $t = -4$ to all observations. Simulated EITC amounts trend similarly in low, middle, and high bunching areas prior to child birth, supporting Assumption 2b. In the year of child birth, the simulated credit jumps significantly in high bunching areas relative to low bunching areas, showing that individuals in high-knowledge areas make an active effort to maintain earnings closer to the refund-maximizing level after having a child.\textsuperscript{57} We estimate the magnitude of the impact using difference-in-differences specifications analogous to those used in the movers event studies in Figure 4a, clustering standard errors at the ZIP-3-by-birth-year level. EITC refunds increase by $85.4$ (4.7\%) more from the year before to the year of child birth in the highest bunching decile relative to the lowest bunching decile.

In Figure 14a, we expand the analysis to include all neighborhoods by plotting the change in the simulated EITC from the year before birth (event year -1) to the year of birth (event year 0) vs. the level of sharp bunching in the individual’s ZIP-3 in the year of birth, which we denote by $b_{ct0}$. In this figure, we include all wage earners with incomes in the EITC-eligible range, as in Figure 12, as well as those with zero earnings (whose simulated credit is zero) to incorporate extensive margin responses. Consistent with the preceding evidence, individuals living in areas with higher $b_{ct0}$ (i.e., higher knowledge) have significantly larger increases in simulated EITC amounts around child birth. A one percentage point (0.58 standard deviation) increase in $b_{ct0}$ leads to a $26.5$ increase in the EITC after child birth, an effect that is statistically significant with $p < 0.0001$ with standard errors clustered at the ZIP-3-by-birth-year level.

\textit{Endogenous Sample Selection.} Our child birth analysis sample makes two restrictions that could potentially lead to selection bias, thereby violating Assumption 2b. The first restriction is that we can only link parents to children they claim as dependents.\textsuperscript{58} Because the decision to claim a child could be endogenous to knowledge about the EITC, this could also potentially bias our estimates through two channels. First, if a child is never claimed by any parent as a dependent, he or she is not included in our sample. In practice, over 97\% of children are claimed as dependents on

\textsuperscript{57}The slight divergence between the series in year -1 may occur because individuals in high-bunching areas keep their jobs prior to birth, recognizing that they will soon be eligible for a large EITC refund.

\textsuperscript{58}Importantly, we observe date of birth from social security records. Each child’s birth date is therefore measured independently of parents' tax filing behavior; only our link between parents and children is potentially endogenous to EITC incentives.
a tax return within 4 years of their birth.\textsuperscript{59} Hence, endogeneity arising from whether a child is claimed at all is minimal.\textsuperscript{60} Second, selection bias could arise if the person who claims a child is endogenously selected, e.g. if the family member who gets the highest EITC refund claims the child in high-knowledge areas. Such selection bias should be manifested in the period prior to child birth, as it would produce differences in simulated EITC credit amounts in event year -1 in Figures 12a and 13. Stated differently, we find sharp changes in earnings behavior within individuals around child birth. Bias can arise only if the decision to claim a child is related to changes in earnings around the time of child birth differentially across areas. While we cannot directly rule out such dynamic selection patterns, they are unlikely to produce a sharp break in earnings behavior only in the year of child birth given the smooth and relatively parallel dynamics of earnings across areas in prior years. Moreover, selection biases are unlikely to explain the asymmetric impacts of past neighborhoods for movers reported above.

The second restriction we impose above is to exclude individuals who report non-zero self-employment income in order to isolate wage earnings responses. If the choice to report self-employment income varies endogenously across areas, this restriction could also bias our estimates of the impact of the EITC on wage earnings.\textsuperscript{61} To address this concern, we analyze changes in W-2 earnings around child birth for the full sample, including both wage-earners and the self-employed. We calculate the simulated EITC credit based purely on W-2 wage earnings even if the individual has self-employment income to isolate wage earnings responses. Figure 14b shows that the relationship between sharp bunching and the change in EITC amounts around child birth remains highly significant when the self-employed are included, with a point estimate of $19.4.\textsuperscript{62} We use this technique to adjust for potential endogenous selection by including self-employed individuals and computing EITC amounts based on W-2 earnings in all the remaining

\textsuperscript{59}We compute this statistic by comparing the total number of dependents claimed in the tax data to total births in the U.S. from vital statistics. This ratio is approximately 99% for births between 2000 to 2005. This figure slightly overstates our true coverage rate because it ignores children who immigrated to the U.S. and are claimed by their parents. Comparing vital birth statistics to all individuals recorded in the tax data, we estimate that immigration at young ages adds less than 0.5% per year to the size of a cohort, and hence obtain a lower bound of 97% for the fraction of individuals claimed.

\textsuperscript{60}Most children are claimed very quickly after child birth presumably because knowledge that claiming children yields large tax credits is widespread. Conditional on claiming a child within four years of his or her birth, we find no evidence that parents living in ZIP-3’s with high levels of sharp bunching claim a child more quickly after birth.

\textsuperscript{61}For instance, suppose individuals in high-bunching areas are more likely to fabricate self-employment income after child birth if their wage earnings put them in the phase-in region rather than the plateau. By excluding those with self-employment income, we would artificially obtain a sample that exhibits more mass in the wage earnings distribution around the plateau in high-bunching areas.

\textsuperscript{62}The magnitude of the coefficient is attenuated because we effectively miscalculate EITC amounts for self-employed individuals by using only their wage earnings to simulate their credits. We discuss how this attenuation bias can be corrected when computing elasticities below.
Robustness Checks. In Table III, we assess the robustness of the result in Figure 14b to alternative specifications of the form:

\[ EITC_{ict} = \alpha + \beta_1 b_{ct0} + \beta_2 \text{post} + \beta_3 \text{post} \times b_{ct0} + \gamma X_{ict} + \varepsilon_{ict} \]

We estimate (5) using only observations in the year before and the year of child birth, \( t \in \{-1, 0\} \). In this equation, \( EITC_{ict} \) denotes the simulated credit individual \( i \) in ZIP-3 \( c \) obtains in event year \( t \), \( \text{post} \) denotes an indicator for the year of child birth \( (t = 0) \), and \( X_{ict} \) denotes a vector of covariates. The coefficient of interest, \( \beta_3 \), measures the impact of a 1 percentage point increase in sharp bunching \( b_{ct0} \) on the change in the simulated credit from the year before to the year after birth. Standard errors are clustered at the ZIP-3-by-birth-year level to account for potential correlation in earnings across residents of an area. Column 1 of Table III reports \( \beta_3 \) for the specification in Figure 14b (with no controls \( X_{ict} \)) as a reference.\(^{63}\) Column 2 replicates column 1, restricting the sample to individuals working at firms with more than 100 employees (based on the number of W-2’s). We continue to find a highly significant impact in this subgroup, confirming that these changes are not driven purely by manipulation of reported income. The magnitude of the effect is smaller because this specification excludes those with zero earnings from the sample, eliminating extensive margin responses. Column 3 adds ZIP-3 fixed effects interacted with the post indicator, so that \( \beta_3 \) is identified purely from variation in \( b_{ct0} \) over time within areas.\(^{64}\) The coefficient on \( b_{ct0} \) remains large and highly significant in this specification, showing that unobservable differences across areas do not drive our findings.

A simple placebo test for our child birth research design is to examine changes in behavior for individuals having their third child instead of first child. Individuals with two or three children were eligible for the same EITC credit during the years of child birth that we analyze (2000 to 2005). The series in triangles in Figures 14a and 14b plots changes in simulated credit amounts (again using the one-child EITC schedule) from the year before to the year of the birth of a third child. Reassuringly, the relationship between neighborhood sharp bunching and changes in simulated credits around the birth of the third child is a precisely estimated zero, as shown in column 4 of Table III. This result confirms that the impacts of child birth on wage earnings do not

\(^{63}\) In a balanced panel, the estimate of \( \beta_3 \) in equation (5) is identical to the estimate obtained from a univariate regression of the change in EITC amounts on \( b_{ct0} \) as in Figure 14b.

\(^{64}\) Allowing for ZIP-3×post fixed effects permits every ZIP-3 to have a different trend in EITC around child birth. Hence, the only remaining source of identification for \( \beta_3 \) comes from comparing individuals who give birth in different years within a ZIP-3.
vary systematically across neighborhoods in the absence of changes in EITC incentives, supporting Assumption 2b.

The estimated impact from the child birth design of a $19.4 increase in the simulated credit per percentage point of sharp bunching is similar to the corresponding cross-sectional estimate in Figure 11a of $15.9. As discussed in Section II, cross-neighborhood comparisons incorporate endogenous changes in wage rates offered by firms as a result of shifts in the labor supply curve induced by the EITC. In contrast, changes in labor supply around child birth do not affect the equilibrium wage rate a new parent is offered as long as labor markets for parents and non-parents are not segregated. The fact that both research designs uncover significant and relatively similar impacts of the EITC on earnings suggests that general equilibrium feedback effects do not fully undo the partial-equilibrium changes in earnings behavior induced by the EITC. However, we cannot definitively identify the magnitude of general equilibrium effects because our cross-sectional estimate relies on a strong assumption for identification, namely that low and high bunching areas would have comparable wage earnings distributions absent the EITC (Assumption 2a).

**Decomposition of Phase-In, Phase-Out, and Extensive Margin Responses.** The welfare consequences of the EITC depend on whether the higher concentration of earnings around the refund-maximizing plateau of the EITC schedule comes from increased earnings for those who would have been in the phase-in region or reduced earnings from those who would have been in the phase-out region. To isolate the phase-in response, we define a “simulated phase-in credit” as the phase-in portion of the EITC schedule (for a single earner with one child) combined with a constant refund above the first kink at the refund-maximizing level. Analogously, we define a “simulated phase-out credit” as the phase-out portion of the schedule combined with a constant refund below the second kink at the refund-maximizing level. Appendix Figure 4 depicts these two schedules. The simulated phase-in credit is a convenient summary statistic for earnings increases in the phase-in region because it grows when individuals raise their earnings in the phase-in but is unaffected by changes in earnings in the plateau and phase-out regions. The simulated phase-in credit asks, “How would behavioral responses affect refund amounts if the EITC stayed constant at its maximum level and was never phased out?” The simulated phase-out credit similarly isolates changes in earnings behavior in the phase-out region. We define both simulated credits based purely on wage earnings (but include self-employed individuals in the sample) as above.

---

65 Formally, we define the simulated phase-in credit as \( \min(0.34 \times z_i, 3050) \) and the phase-out credit as \( \max(3050 - 0.16 \times \min(z_i - 16450, 0), 0) \).
Figure 15a plots changes in the simulated phase-in and phase-out credits around child birth vs. the degree of sharp bunching. The corresponding regression coefficients are reported in columns 5 and 6 of Table III. By construction, the slopes of the two coefficients sum to the slope for the full EITC credit schedule in column 1. Figure 15a shows that $14.2/19.4 = 73\%$ of the increase in EITC refunds in high-bunching areas comes from the phase-in region. As a result, the EITC program is successful in increasing wage earnings for low-income individuals despite creating high marginal tax rates in a broad part of income distribution.

Next, we separate intensive and extensive margin responses. To analyze extensive margin responses, we define “working” as having positive W-2 earnings in a given year. We use the full sample (including non-workers, self-employed individuals, and wage earners) for this analysis. Figure 15b plots the change in the fraction of individuals working from the year before to the year of child birth vs. sharp bunching; the corresponding regression coefficient is reported in column 7 of Table III.\textsuperscript{66} Consistent with prior studies, we find significant extensive margin responses. Individuals living in areas with high levels of sharp bunching are more likely to continue working after they have a child than those living in areas with little sharp bunching. To gauge the extent to which extensive margin responses contribute to the increase in EITC refunds, we assume that extensive margin entrants earn the average EITC refund in the child birth sample conditional on working ($1,075$). Under this assumption, the extensive margin contributes $5.8/19.4 = 29\%$ to the increase in EITC refunds, as shown in column 8 of Table III.\textsuperscript{67}

Finally, in column 9 of Table III, we analyze the number of W-2’s per individual, which is a proxy for the number of distinct jobs an individual held over the year. A one percentage point increase in sharp bunching leads to a 0.017 (0.018 SD) increase in the number of W-2’s filed after child birth. Hence, part of the increase in earnings in the phase-in region comes from individuals taking additional part-time jobs.\textsuperscript{68} Adjustment in part-time jobs could explain why earnings responses to the EITC are larger in the phase-in than the phase-out. In our child birth sample, individuals in the phase-in have 1.61 W-2’s per person, at which they earn $2,300 per job on average. Those

\textsuperscript{66}The mean fraction of individuals working in this sample is 82\% in the year before child birth and 84\% in the year of child birth. The fraction working increases around child birth because this sample includes predominantly young, unmarried women who are entering the labor force and because our definition of “working” is defined as having any earnings over a year.

\textsuperscript{67}We can obtain a non-parametric upper bound on the extensive margin response by assuming that all individuals who enter on the extensive margin earn the maximum EITC refund (i.e., choose a level of earnings in the plateau). This calculation reveals that the extensive margin response accounts for at most 90\% of the total response. Hence, we can be confident that the EITC induces responses on both the intensive and extensive margin; however, the relative magnitude of intensive and extensive responses is less clear.

\textsuperscript{68}It is difficult to determine exactly what fraction of the response comes from additional jobs because we would need an estimate of earnings at the marginal job.
in the phase-out have 1.42 W-2’s with mean earnings of $14,300 per W-2. Because they work more small, part-time jobs, individuals in the phase-in may be able to change their earnings more easily than those in the phase-out. An alternative explanation for larger phase-in elasticities is that current perceptions of the EITC schedule focus on phase-in incentives more than the phase-out incentives. We cannot distinguish between these explanations with our research design.

VI Policy Calculations

In this section, we use our estimates to quantify the impacts of the EITC in two ways. First, we calculate the elasticities implied by our analysis in a neoclassical model to gauge the magnitudes of the earnings responses documented above. Second, we characterize the impacts of the EITC on the wage earnings distribution and poverty rates.

Earnings Elasticities. One of the main lessons of our study is that the impacts of tax policies cannot be characterized using a single elasticity, as the behavioral responses we have documented do not conform to the predictions of traditional labor supply models. Nevertheless, to help gauge magnitudes and revenue consequences, we calculate the elasticity that would generate the increase in EITC refunds we observe under a neoclassical, frictionless model.\textsuperscript{69}

Panel A of Table IV reports elasticity estimates for wage earners. The first column reports the intensive-margin elasticity that would generate an increase in EITC refunds commensurate to the empirical estimates above. We compute these elasticities as follows. With a standard iso-elastic labor supply function, a frictionless model with elasticity $\varepsilon$ implies

$$\log(z + \Delta z) - \log(z) = \varepsilon \cdot \log(1 - \tau)$$

where $\tau$ is the actual marginal tax rate an individual faces because of the EITC, $z$ is the level earnings when the EITC marginal tax rate is perceived to be zero, and $z + \Delta z$ is earnings when the EITC marginal tax rate is accurately perceived to be $\tau$.\textsuperscript{70} The change in the EITC refund

\textsuperscript{69}Our technique is analogous to that used by Ashenfelter (1983) to estimate the elasticities implied by behavioral responses to the Negative Income Tax (NIT). Ashenfelter calculates the elasticities that would generate the observed changes in NIT participation rates under a constant-elasticity labor supply model; here we calculate the elasticities that would generate the observed increase in EITC refunds in the same model. Note that in a frictionless labor supply model, the increase in EITC refunds would come primarily from a point mass in the wage earnings distribution at the kink points of the EITC schedule, which is not what we observe empirically. This is why our estimate does not represent the actual structural labor supply elasticity implied by the data.

\textsuperscript{70}For simplicity, this equation assumes that individuals remain on the interior of the budget segment when they increase earnings by $\Delta z$. Accounting for the kinks in the EITC schedule significantly complicates the calculations and has little impact on the estimated elasticities.
induced by the earnings response is:

\[-\tau \cdot \Delta z = -\tau \cdot [(1 - \tau)^z - 1] \cdot z\]

and the mean increase in EITC refunds due to behavioral responses in the phase-in and phase-out regions is

\[\Delta EITC = -\phi_1 \tau_1 \cdot [(1 - \tau_1)^z - 1] \cdot z_1 - \phi_2 \tau_2 \cdot [(1 - \tau_2)^z - 1] \cdot z_2\]

where \(\phi_1 = 26.9\%\) and \(\phi_2 = 22.1\%\) denote the fraction of individuals in the phase-in and phase-out regions in the year after birth in our child birth sample.\(^7\) \(\tau_1 = -34\%\) and \(\tau_2 = 16\%\) denote the phase-in and phase-out marginal tax rates and \(z_1 = $5,725\) and \(z_2 = $23,216\) denote the mean earnings levels in the phase-in and phase-out regions.

We calculate the change in EITC refunds \(\Delta EITC\) as follows. We begin from our estimate that a one percentage point increase in sharp bunching increases mean EITC amounts by $19.4, shown in column 1 of Table III. This estimate uses the full sample, in which 10.8% of individuals are self-employed. As in our model, we assume that self-employed individuals do not respond along the wage earnings margin (as adjusting self-employment income is less costly). Therefore, the impact of the EITC on the treated (i.e., the wage earners) is \(19.4 \cdot (1 - .108) = $21.7.\) The average ZIP-3 in our sample has a level of sharp bunching \(b_{ct}\) that is 1.34 percentage points higher than a bottom decile bunching area. Under our maintained assumption that areas in the bottom decile of bunching exhibit no behavioral response to the EITC, the increase in EITC refunds for the average neighborhood in the U.S. relative to areas with no response is \(\Delta EITC = 1.34 \times 21.7 = $29.1.\) Substituting this value into (6) and solving for \(\varepsilon\) yields \(\varepsilon = 0.10\) (Table IV, Column 1, Row 1).

This estimate of \(\varepsilon\) assumes that the earnings elasticity is the same in the phase-in and phase-out regions of the schedule. However, as demonstrated in Figure 15a, responses in the phase-in and phase-out regions are quite different in magnitude. In columns 2 and 3 of Table IV, we estimate the elasticities in the phase-in and phase-out regions separately using the estimates from columns

\(^7\)In our child birth sample, 26.9+22.1=49% of individuals have income below the end of the phase-out region. For simplicity, we abstract from the constant marginal tax rate in the plateau region and assume that those in the bottom half of the plateau are in the phase-in and those in the upper half of the plateau are in the phase-out in terms of the change in the marginal incentives they face.
5 and 6 of Table III. We estimate these elasticities using the formulas

\[
\begin{align*}
\Delta \text{Phase-in EITC} &= -\phi_1 \cdot \tau_1 \cdot [(1 - \tau_1)^{\varepsilon_1} - 1] \cdot z_1 \\
\Delta \text{Phase-out EITC} &= -\phi_2 \cdot \tau_2 \cdot [(1 - \tau_2)^{\varepsilon_2} - 1] \cdot z_2
\end{align*}
\]

Computing the changes in EITC amounts as above, we obtain \( \varepsilon_1 = 0.14 \) for the phase-in elasticity and \( \varepsilon_2 = 0.06 \) for the phase-out elasticity.

Finally, in column 4 of Table IV, we report estimates of extensive margin elasticities. We define the participation tax rate \( \tau_{ext} \) as the mean EITC refund as a percentage of mean income conditional on working. We then use the estimate in column 7 of Table III and define \( \hat{\varepsilon}_{ext} \) as the log change in participation rates (starting from the sample mean) divided by the log change in the net-of-participation-tax rate. This yields an estimate of \( \hat{\varepsilon}_{ext} = 0.10 \). In interpreting this elasticity, it is important to note that our definition of “working” is having positive earnings at any time within a year. While this annual concept is what matters for optimal policy in an annual tax system, it is likely that the EITC induces larger extensive margin responses at higher frequencies, e.g. at the weekly level. This could explain why our estimate of the extensive margin elasticity is slightly smaller than estimates in prior studies using survey data (e.g., Eissa and Liebman 1996).

The preceding elasticities apply to the U.S. as a whole given the average level of knowledge about the EITC schedule in the economy between 2000 and 2005. In row 2 of Table IV, we report elasticities for areas in the top decile of sharp bunching \( b_{ext} \), i.e. the areas with the highest levels of knowledge in our sample. We calculate these elasticities using the same method as above, but define the increase in EITC refunds as the difference-in-differences between EITC refunds in the top vs. bottom decile in the year after birth vs. before birth. The elasticities are roughly 5 times larger in areas in the top bunching decile relative to the country as whole. These calculations suggest that behavioral responses may grow significantly as knowledge about the EITC’s structure continues to spread across the U.S., increasing earnings levels as well as expenditures on the program.

Panel B of Table IV replicates Panel A including self-employment income. It reports total earnings elasticities, calculated exactly as in Panel A using regressions analogous to those in columns 1 and 8 of Table III but using total earnings instead of wage earnings. These regression estimates are reported in Appendix Table I. Total earnings elasticities are much larger because self-employed individuals exhibit large responses to the EITC, especially in high bunching areas. The mean earnings elasticity is 0.22 in the U.S. as a whole and 0.95 in the top bunching decile. Even though less than a fifth of EITC claimants are self-employed, they account for a substantial fraction of the
increase in EITC refunds via behavioral responses. As shown in our companion paper (Chetty et al. 2012), much of this response is driven by non-compliance. Reducing the behavioral response due to non-compliance through auditing or changes in reporting requirements for self-employment income may make the EITC more effective at raising true earnings.

**Impacts on the Income Distribution.** We characterize the impact of the EITC on the wage earnings distribution by calculating the fraction of wage-earners in our cross-sectional analysis sample below the poverty line and other income thresholds $\bar{z}$.\(^{72}\) We begin by estimating the causal impact of the EITC on the fraction of individuals below each threshold $F(\bar{z})$ using our child birth design. Let $t = 0$ denote the year of child birth and $t = -1$ the year before child birth. Let $b_d = 1$ denote ZIP-3-by-birth-year cells in the first decile of the bunching distribution. We define the treatment effect of the EITC using a difference-in-differences estimator:

$$
(7) \quad \Delta F(\bar{z}) = [F(\bar{z}, t = 0) - F(\bar{z}, t = -1)] - [F(\bar{z}, t = 0|b_d = 1) - F(\bar{z}, t = -1|b_d = 1)]
$$

The first difference is the change in the fraction of individuals below the poverty line in the full population; the second is the same difference within neighborhoods in the lowest bunching decile. We estimate the fraction who would have wage earnings below threshold $\bar{z}$ absent the EITC in the full population as $F(\bar{z}) - \Delta F(\bar{z})$.

We characterize the impact of the EITC on the average earnings distribution between 2000 and 2005, the period over which we estimate the treatment effect $\Delta F(\bar{z})$ using our child birth sample. The first row of Table V shows our estimate of $F(\bar{z})$ without the EITC for various multiples of the poverty line. For instance, we estimate that 31.9% of wage-earners in our cross-sectional analysis sample – which consists of EITC-eligible households with children – would be below the poverty line without the EITC. In the second row, we add in EITC payments based on the individual’s wage earnings, marital status, and number of dependents.\(^{73}\) We assume that all eligible households claim their benefit and hold wage earnings for each household fixed at the same level as in the first row. The difference between the first and second rows thus reflects the mechanical effect of EITC payments on post-tax incomes. EITC payments shift the income distribution upward significantly; the fraction below the poverty line falls to 22.0%.\(^{74}\) The third row reports statistics

\(^{72}\) We use the official poverty line in each year from 2000-2005 (to match the years of the birth sample) corresponding to the individual's marital status and number of children.

\(^{73}\) When making this calculation, we assume that the treatment effect $\Delta F(\bar{z})$ is constant in percentage terms across all subgroups.

\(^{74}\) As 15% of all households with children in the U.S. are EITC eligible, the EITC reduces overall poverty rates in the population by approximately 2 percentage points.
for the observed post-EITC income distribution in the aggregate economy. This row incorporates behavioral responses to the EITC on top of the mechanical effects in the second row. Behavioral responses to the EITC further increase incomes at the lowest levels, as workers response to the marginal subsidy on the phase-in. Taking behavioral responses into account, the fraction below the poverty line with the EITC is 21.0%.

In the last row of Table V, we consider the effect of increasing knowledge of the EITC everywhere to the level observed in the highest sharp bunching decile. This row asks, “How would the EITC affect the earnings distribution in the U.S. if knowledge about the schedule were at the level in the highest bunching decile?” We estimate this effect by recalculating (7), replacing the first term with the CDFs in the top bunching decile instead of the full sample. We then add this causal effect back to the counterfactual distribution calculated in the first row of Table V and recompute EITC refund amounts. The increased level of knowledge triples the behavioral response to the EITC, further lowering the fraction below the poverty line to 19.6%.

Table V yields three main lessons. First, the impacts of the EITC on inequality come largely through its mechanical effects rather than behavioral responses in the nation as a whole. Second, behavioral responses tend to reinforce the mechanical effects of the EITC in raising incomes of the lowest earning households in the U.S. For instance, the fraction earning less than half the poverty line – which is near the end of the phase-in region – falls from 13.7% to 9.4% due to the mechanical transfer and falls further to 8.2% because individuals in the phase-in raise their earnings. In contrast, behavioral responses to the disincentive effects of the EITC in the phase-out region of the schedule have much smaller impacts: the fraction earning less than 200% of the poverty line falls from 77.3% to 71.1% due to the mechanical effect, but rises to only 71.3% when incorporating behavioral responses. Third, more than a decade after the EITC was implemented in its current form, the aggregate response to the EITC still comes from a relatively small subset of neighborhoods in the U.S. in which behavioral responses are quite large. However, knowledge about the EITC – as measured by the level of sharp bunching – is still rising sharply, as shown in Figure 7a. As knowledge about the EITC continues to spread through the U.S., the EITC is likely to have larger effects on the aggregate income distribution.

VII Conclusion

A growing literature finds that many policies have diffuse effects on economic behavior that are inconsistent with neoclassical models because of inattention and other frictions. Identifying diffuse
impacts has thus emerged as one of the major challenges for applied work on policy evaluation. This paper has developed a new method of addressing this challenge by using differences across neighborhoods in knowledge about the policy to obtain counterfactuals for diffuse responses. We apply this method to characterize the impacts of the EITC on earnings behavior by using the degree of sharp bunching at the refund-maximizing income level by the self-employed as a proxy for local knowledge about the EITC schedule. We find that areas with higher levels of knowledge exhibit significantly more mass in the wage earnings distribution around the EITC plateau. In addition, changes in marginal incentives due to child birth have larger impacts on wage earnings behavior in areas with higher levels of knowledge about the EITC.

The wage earnings response comes primarily from intensive-margin increases in earnings by individuals in the phase-in region. As a result, behavioral responses to the EITC reinforce its direct impacts in raising the incomes of low-income families with children. Overall, we conclude that the EITC has increased earnings and net income levels among low-income families in the U.S., with especially large impacts in areas with a high density of EITC claimants.

Our analysis can be extended and generalized in several dimensions. Most directly, one could use the counterfactuals developed here to study the impacts of the EITC on other behaviors, such as contribution to tax-deferred savings accounts, family formation, and earnings dynamics. One could also use a similar approach to develop proxies for knowledge about other policies and study their impacts. For instance, several studies have documented sharp bunching around the kinks of the Social Security earnings test schedule (Friedberg 2000, Gruber and Orszag 2003, Haider and Loughran 2008). Using spatial variation in such bunching, one may be able to characterize the impacts of Social Security incentives on retirement behavior in the U.S. Similar techniques could also shed light on the impacts of corporate tax credits, which create sharp incentives for manipulation around thresholds (e.g., Goolsbee 2004), but may affect real investment decisions more diffusely. More generally, using low-knowledge groups as counterfactuals could help uncover the impacts of a variety of important policies whose effects have proven difficult to characterize with traditional research designs.
References


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<tr>
<th>Variable</th>
<th>Mean (1)</th>
<th>Std. Dev. (2)</th>
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</thead>
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<td><strong>Income Measures</strong></td>
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<tr>
<td>Total Earnings</td>
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<tr>
<td>Wage Earnings</td>
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<td>Self-Employment Income</td>
<td>$1,770</td>
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<tr>
<td>Indicator for Non-Zero Self-Emp. Income</td>
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<tr>
<td>Number of W-2's</td>
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<td>EITC Refund Amount</td>
<td>$2,543</td>
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<td>Tax Professional Usage</td>
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<tr>
<td>Age</td>
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<tr>
<td>Number of Children</td>
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<tr>
<td>Married</td>
<td>30.3%</td>
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<tr>
<td>Female (for single filers)</td>
<td>73.0%</td>
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<td><strong>Neighborhood (ZIP-3) Characteristics</strong></td>
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<tr>
<td>Self-Emp. Sharp Bunching</td>
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<tr>
<td>EITC Filer Density</td>
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<td>State EITC Top-Up Rate</td>
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<td><strong>Number of Observations</strong></td>
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Notes: This table reports summary statistics for the cross-sectional sample, which includes primary filers in our core sample (defined in Section 3) who file a tax return, report one or more children, and have income in the EITC-eligible range. We restrict the sample to 1999-2009, the years for which we have W-2 earnings data. Total earnings, which includes wage earnings and self-employment earnings, is the earnings measure used to calculate EITC refunds. Self-employment income is income reported on Schedule C. Wage earnings are earnings reported on Form W-2 by employers. We trim all income measures at -$20K and $50K. Tax professional usage is the fraction of individuals using a third-party tax preparer. Age is defined as of December 31 of a given tax year. Number of children is number of EITC-eligible dependents claimed on Schedule EIC; for those who do not file Schedule EIC, it is the number of non-elderly dependents claimed on Form 1040. Statistics for neighborhood variables weight ZIP-3 level means by the number of EITC-eligible individuals with children in the cross-sectional analysis sample. Self-employed sharp bunching is the fraction of EITC-eligible filers with children who both report total earnings within $500 of the first kink point in the EITC schedule and have non-zero self-employment earnings. EITC filer density is the number of EITC-eligible filers (measured in 1000's) per square mile in tax year 2000. State EITC top-up rate is state EITC as a fraction of the federal credit.
TABLE II
Cross-Sectional Correlates of Sharp Bunching

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<th>Dependent Variable:</th>
<th>Self-Employed Sharp Bunching Rate in ZIP-3 (%)</th>
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<tr>
<td>EITC Filer Density in ZIP-3</td>
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<td>State Non-Compliance Rate</td>
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</tbody>
</table>

Notes: Each column reports estimates from an OLS regression run at the ZIP-3 level, weighted by the number of individuals in each ZIP-3 in the cross-sectional analysis sample. Standard errors are reported in parentheses. EITC filer density is the number of EITC filers (measured in 1000's) per square mile in the ZIP-3. Tax professional usage is the fraction of EITC filers who use a professional tax preparer in the ZIP-3. Google search intensity for "tax" is the fraction of all Google searches in the ZIP-3 for phrases that include the word "tax" divided by standard deviation of this measure, so that the variable is scaled in standard deviation units. State EITC top-up rate is the size of the state EITC top-up as a fraction of the federal EITC; states without a state EITC are coded as zero. State non-compliance rate is the fraction of non-EITC-eligible individuals in a state with a difference between reported and corrected income greater than $1,000; this variable is measured using data from the 2001 IRS National Research Program audit data. The specification in column 8 is estimated at the state level because the non-compliance variable is only available by state even though it may vary locally. Note that state EITC top-up is also measured at the state level, but since that variable does not vary within state, we run the regression at the ZIP-3 level and cluster standard errors by state. The demographic controls include the percentage of the population that is foreign-born, white, black, Hispanic, Asian, and other. We use data from year 2000 in some specifications because Census data are available only in 2000; we use data from year 2008 in other specifications because Google search intensity was high only in more recent years.
### TABLE III

Impacts of EITC on Wage Earnings: Regression Estimates from Child Birth Design

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Simulated EITC Refund</th>
<th>Phase-in vs. Phase-out</th>
<th>Extensive Margin</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline Firms Only</td>
<td>Large Firms Only</td>
<td>Placebo Test: 3rd Child</td>
</tr>
<tr>
<td>ZIP-3 Self-Emp.</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sharp Bunching (%)</td>
<td>(1) $19.4 $14.4 $34.7</td>
<td>(2) $14.2 $14.2 $14.2</td>
<td>(3) $34.7 $5.2 $0.54%</td>
</tr>
<tr>
<td></td>
<td>(1.61) (1.14) (3.20)</td>
<td>(1.55) (1.55) (0.63)</td>
<td>(0.69) (0.69) (0.05)</td>
</tr>
<tr>
<td>ZIP-3 by Post-Birth</td>
<td>x</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>29.96 13.20 29.96 10.07</td>
<td>29.96 29.96 29.96 29.96</td>
<td>29.96 29.96 29.96</td>
</tr>
<tr>
<td>Mean Level of Dep. Var. in Year Before Birth</td>
<td>$1,038 $1,209 $1,038 $899</td>
<td>$1,038 $1,038</td>
<td>$960 1.78</td>
</tr>
</tbody>
</table>

Notes: All specifications are estimated using the child birth sample, which includes individuals in the core sample who had their first child between 2000 and 2005, using only the year before and the year of child birth. All columns include all individuals (wage earners, self-employed, and non-workers). Each column reports estimates from an OLS regression of the outcome on the level of sharp bunching in the ZIP-3-by-year cell in which the individual gives birth to his or her first child, an indicator for the post-birth year, and an interaction of sharp bunching and the indicator for the post-birth year. The table reports coefficients on the interaction term, which can be interpreted as the impact of a one percentage point increase in sharp bunching on the change in the outcome around child birth. Standard errors, clustered at the ZIP-3-by-birth-year level, are reported in parentheses. In column 1, the dependent variable is the simulated EITC refund. To calculate the simulated EITC refund, we apply the one-child EITC schedule for single filers to total household W-2 earnings, regardless of the household's actual structure and self-employment income. Column 2 replicates column 1, restricting the sample to individuals whose W-2 forms are all issued by firms with 100 or more employees in a given year. Column 3 adds ZIP-3 fixed effects to the specification in column 1. Column 4 replicates column 1 using individuals having 3rd births instead of 1st births (for whom there is no change in EITC in tax years 2000-2005) as a placebo test. The dependent variable in column 4 is again the one-child simulated EITC. Columns 5 and 6 decompose the response into the phase-in and phase-out regions. In column 5, the dependent variable is the simulated phase-in credit, which is calculated based on W-2 earnings using the schedule shown in Appendix Figure 4a. In column 6, the dependent variable is the simulated phase-out credit, calculated based on W-2 earnings using the schedule shown in Appendix Figure 4b. The estimates in columns 5 and 6 mechanically sum to the estimate reported in column 1. The dependent variable in column 7 is an indicator for having positive W-2 wage earnings. The dependent variable in column 8 is this indicator multiplied by the average EITC amount for wage earners conditional on working, which is $1,075 in this sample. The estimate in this column can be used to calculate the fraction of the response in column 1 that is due to extensive margin responses. The dependent variable in column 9 is the number of W-2 forms of the individual parent (not the tax return). The bottom row displays the average level of the dependent variable in the year before birth.
TABLE IV
Elasticity Estimates Based on Change in EITC Refunds Around Birth of First Child

<table>
<thead>
<tr>
<th></th>
<th>Mean Elasticity</th>
<th>Phase-in Elasticity</th>
<th>Phase-out Elasticity</th>
<th>Extensive Elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
</tbody>
</table>

A. Wage Earnings

|                                |                 |                    |                      |                      |
|                                | 0.10 (0.008)    | 0.14 (0.011)       | 0.06 (0.006)         | 0.10 (0.009)         |
| Elasticity in U.S. 2000-2005   |                 |                    |                      |                      |
| Elasticity in top decile ZIP-3s| 0.46 (0.017)    | 0.58 (0.021)       | 0.30 (0.021)         | 0.59 (0.033)         |

B. Total Earnings

|                                |                 |                    |                      |                      |
|                                | 0.22 (0.013)    | 0.34 (0.020)       | 0.08 (0.004)         | 0.18 (0.012)         |
| Elasticity in U.S. 2000-2005   |                 |                    |                      |                      |
| Elasticity in top decile ZIP-3s| 0.95 (0.026)    | 1.32 (0.036)       | 0.34 (0.012)         | 1.05 (0.039)         |

Notes: The first panel reports elasticities using wage earnings responses estimated in Table III; the second panel reports elasticities using total earnings responses (including self-employment income) estimated in Appendix Table I. Standard errors, reported in parentheses, are calculated using the corresponding standard errors in Table III and Appendix Table I. In each panel, the first row reports the mean elasticity implied for the U.S. as a whole, while the second row reports the elasticity in the top bunching decile of ZIP-3-by-year cells. The identifying assumption in both cases is that the elasticity is zero in the bottom bunching decile. Column 1 reports the intensive margin elasticity required in a neoclassical model of frictionless optimization to generate the increase in EITC amounts around child birth estimated in column 1 of Table III for Panel A and column 1 of Appendix Table I for Panel B. Column 2 reports the elasticity in the phase-in range required to generate the increase in the phase-in EITC amounts estimated in column 5 of Table III for Panel A and column 4 of Appendix Table I for Panel B. Column 3 reports the elasticity in the phase-out range required to generate the increase in the phase-out EITC amounts estimated in column 6 of Table III for Panel A and column 5 of Appendix Table I for Panel B. Column 4 reports estimates of participation elasticities using the estimates reported in column 7 of Table III and column 6 of Appendix Table I. The top decile elasticities are calculated to match the increase in EITC amounts around child birth in decile 10 relative to decile 1. See the text for additional details on the calculation of these elasticities.
### TABLE V
Impact of EITC on Wage Earnings Distribution of EITC-Eligible Households

<table>
<thead>
<tr>
<th>Percent of EITC-Eligible Households Below Threshold</th>
<th>(1) 50% of Poverty Line</th>
<th>(2) 100% of Poverty Line</th>
<th>(3) 150% of Poverty Line</th>
<th>(4) 200% of Poverty Line</th>
</tr>
</thead>
<tbody>
<tr>
<td>No EITC Counterfactual</td>
<td>13.71%</td>
<td>31.91%</td>
<td>54.31%</td>
<td>77.27%</td>
</tr>
<tr>
<td>EITC with No Behavioral Response</td>
<td>9.40%</td>
<td>21.95%</td>
<td>42.14%</td>
<td>71.11%</td>
</tr>
<tr>
<td>EITC with Avg. Behavioral Response in U.S.</td>
<td>8.16%</td>
<td>21.00%</td>
<td>41.97%</td>
<td>71.29%</td>
</tr>
<tr>
<td>EITC with Top Decile Behavioral Response</td>
<td>6.15%</td>
<td>19.56%</td>
<td>41.99%</td>
<td>71.73%</td>
</tr>
</tbody>
</table>

Notes: This table presents CDF’s of wage earnings distributions under various scenarios. Each column reports the CDF of the income distribution of EITC-eligible wage earners with dependents at various thresholds relative to the Federal Poverty Line (FPL). We calculate the FPL for each observation in our sample based on year, marital status and number of children. The first row shows statistics for the counterfactual wage earnings distribution if there were no EITC. To construct this distribution, we first estimate the causal impact of the EITC on wage earnings using the difference-in-differences estimator around child birth described in equation (7). We then subtract this estimate of the causal impact of the EITC from the CDF of the observed unconditional wage earnings distribution in our sample between 2000-2005. The second row recomputes the CDF in the first row after mechanically adding the EITC payments each household would receive based on its characteristics. The third row reports the observed CDF in our sample using the unconditional post-EITC wage earnings distribution. This row incorporates the effects of both mechanical transfers and behavioral responses to the EITC. The fourth row reports the counterfactual net earnings distribution if the level of information increased in all areas to that of neighborhoods in the highest decile of self-employed sharp bunching in our sample. We estimate this effect by recalculating the difference-in-differences estimate of the causal impact of the EITC using the top bunching decile instead of the full sample. We then add this causal effect back to the counterfactual distribution calculated in the first row and recompute EITC refund amounts.
### APPENDIX TABLE I
Impacts of EITC on Total Earnings: Regression Estimates from Child Birth Design

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Simulated EITC Refund</th>
<th>Placebo Test: 3rd Child</th>
<th>Phase-in vs. Phase-out</th>
<th>Extensive Margin</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline Specification</td>
<td>With ZIP-3 Effects</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ZIP-3 Self-Emp.</td>
<td>$44.2</td>
<td>$47.5</td>
<td>$2.1</td>
<td>$36.9</td>
</tr>
<tr>
<td>Sharp Bunching (%)</td>
<td>(2.60)</td>
<td>(0.99)</td>
<td>(0.86)</td>
<td>(2.39)</td>
</tr>
<tr>
<td>ZIP-3 by Post-Birth</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fixed Effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations (millions)</td>
<td>29.96</td>
<td>29.96</td>
<td>10.07</td>
<td>29.96</td>
</tr>
</tbody>
</table>

Notes: This table replicates selected columns from Table III using total earnings (self-employment income plus wage earnings) to calculate the simulated EITC refund. See Table III for details on the variables and specifications.
Notes: Panel A plots the distribution of total earnings for all individuals in our cross-sectional analysis sample in 2008, which includes primary tax filers who report one or more children and have income in the EITC-eligible range. This and all subsequent distributions are histograms with $1,000 bins centered around the first kink of the EITC schedule. Total earnings is the total amount of earnings used to calculate the EITC and is essentially the sum of wage earnings and self-employment income reported on form 1040. We plot separate distributions for households claiming one child and households claiming two or more children. Panel B repeats Panel A for wage earners, i.e. households who report no self-employment (Schedule C) income in 2008. Each panel also shows the EITC credit schedule for single filers with one and two or more children in 2008 (right scale). The dashed lines depict the income level that maximizes refunds net of other tax liabilities. Married households filing jointly face schedules with the same first kink point, but a plateau region extended by $3,000. In this and all subsequent figures, dollar values are scaled in 2010 real dollars using the IRS inflation adjustment.
FIGURE 2
Self-Employed Sharp Bunching Rates Across Neighborhoods

Notes: This figure plots sharp bunching rates by ZIP-3 in 2008. Self-employed sharp bunching is defined as the fraction of all EITC-eligible households with children in the cross-sectional sample whose total income falls within $500 of the first kink point and who have non-zero self-employment income. We divide the observations into deciles within the 2008 cross-sectional sample. Each decile is assigned a different color on the map, with darker shades representing higher levels of sharp bunching.
FIGURE 3
Earnings Distributions in Lowest vs. Highest Sharp Bunching Deciles

Notes: This figure plots the distribution of total earnings for individuals living in ZIP-3-by-year cells in the highest and lowest deciles of self-employed sharp bunching. Self-employed sharp bunching is defined as the percentage of EITC claimants with children in the ZIP-3-by-year cell who report total earnings within $500 of the first EITC kink and have non-zero self-employment income. We use all years in the cross-sectional analysis sample (1996-2009) in this figure. We divide the observations into deciles after pooling all years of the sample, so that the decile cut points remain fixed across years. The figure includes individuals with both 1 and 2+ children by plotting total earnings minus the first kink point of the relevant EITC schedule, so that 0 denotes the refund-maximizing point.
FIGURE 4
Event Studies of Movers

(a) Self-Employed Sharp Bunching

- Effect of Moving to 10th Decile = 1.93 (0.13)
- Effect of Moving to 1st Decile = -0.41 (0.11)

(b) EITC Refund Amount

- Effect of Moving to 10th Decile = $150.1 (22.5)
- Effect of Moving to 1st Decile = $5.1 (19.0)

Notes: Each panel plots an event study of individuals who move across ZIP-3s. We define event time as the calendar year minus the year of the move, so year 0 is the year in which the individual moves. The figure is drawn using the movers sample, which includes all individuals in our core sample who move across ZIP-3s in any year between 2000 and 2005. If an individual moves more than once, we use only the first move. To construct the figure, we first define the degree of bunching for prior residents of ZIP-3 $c$ in year $t$ as the sharp bunching rate for individuals in the cross-sectional analysis sample living in ZIP-3 $c$ in year $t - 1$. We then divide the ZIP-3-by-year cells into ten deciles of prior residents’ bunching rates by splitting the individual-level observations in the movers sample into ten equal-sized groups. Each figure plots outcomes for individuals who move from ZIP-3-by-year cells in the 5th decile to cells in the 1st, 5th, and 10th deciles. The outcome in Panel A is the rate of self-employed sharp bunching among the movers themselves. The outcome in Panel B is the mean EITC refund for the movers. In both panels, we include only individual-year observations in which the mover has one or more children and has total earnings in the EITC-eligible range. The coefficients and standard errors are estimated using difference-in-differences regression specifications comparing changes from year -1 to 0 for movers to the 10th or 1st deciles with changes for those moving to the 5th decile. See text for details. Standard errors are clustered at the ZIP-3-by-year of move level.
FIGURE 5
Total Earnings Distributions Before and After Move

Notes: These figures plot the distribution of total earnings before and after moving for the three groups of movers shown in Figure 4. Panel A shows the distribution of total earnings relative to the first kink point in the year before the move. Panel B repeats this exercise for the year of the move. As in Figure 2, we include individuals with both 1 and 2+ children by plotting total earnings minus the first kink point of the relevant EITC schedule, so that 0 denotes the refund-maximizing point. See the notes to Figure 4 for details on sample and decile definitions.
FIGURE 6
Impact of Moving to Neighborhoods with Lower vs. Higher Sharp Bunching

Notes: This figure plots changes in EITC refund amounts from the year before the move (event year -1 in Figure 4) to the year after the move (event year 0) vs. changes in the level of residents’ sharp bunching across the old and new ZIP-3s. We define the change in ZIP-3 sharp bunching as the difference between bunching of prior residents of the ZIP-3 where the mover lives before the move and bunching of the ZIP-3 where the mover lives after the move. As in Figure 4, bunching for prior residents of ZIP-3 \( c \) in year \( t \) is defined as the sharp bunching rate in year \( t \) for individuals in the cross-sectional analysis sample living in ZIP-3 \( c \) in year \( t - 1 \). Bunching after the move is defined as the sharp bunching rate in year \( t \) in the mover’s new ZIP-3. To construct the figure, we group individuals into 0.05%-wide bins on changes in sharp bunching and then plot the means of the change in average EITC refund within each bin. The solid lines represent best-fit linear regressions estimated on the microdata separately for observations above and below 0. The estimated slopes are reported next to each line along with standard errors clustered by bin. See the notes to Figure 4 for further details on the sample definitions.
FIGURE 7
Correlates of Sharp Bunching

a) Evolution of Self-Emp. Bunching in Low vs. High EITC-Density Areas

b) Self-Emp. Bunching vs. Fraction of Professionally Prepared Returns in ZIP-3

Notes: Panel A plots sharp bunching rates by year for two groups: ZIP-3s with above-median and below-median EITC filer density. We calculate density as the number of EITC-eligible filers per square mile. We split ZIP-3s into two groups at the median based on their density in 1996 (weighting by the number of individuals in each ZIP-3), and then plot the average level of sharp bunching in each group over time. Panel B plots the relationship between bunching and the fraction of returns filed in each ZIP-3-by-year cell using third-party professional tax preparers. We define the use of a professional tax preparer as reporting either a Tax Preparer TIN (PTIN) or Tax Preparer EIN on Form 1040 and compute the fraction of returns using a professional tax preparer within each ZIP-3-by-year cell in our cross-sectional sample. To construct the plot in Panel B, we split the cross-sectional sample into twenty equal-sized bins based on the fraction of tax prepared returns. Within each bin, we then plot mean sharp bunching for two groups: filers who file their own returns and filers who themselves use a third-party preparer. Coefficients are from OLS regressions estimated at the ZIP-3-by-year level, weighted by the number of individuals in each cell, with standard errors reported in parentheses.
FIGURE 8
Impacts of Child Birth on Reported Self-Employment Income

Notes: These figures are drawn using the child birth sample, which includes individuals from the core sample who give birth to their first child between 2000 and 2005. We classify individuals into deciles of sharp bunching based on the level of sharp bunching for residents of the ZIP-3 they inhabit in the year in which they have a child. Panel A includes only individuals with non-zero self employment income and plots the distribution of total earnings in the year before child birth for individuals in the lowest bunching decile, the distribution in the year of child birth for individuals in the lowest bunching decile, and the distribution in the year of child birth for individuals in the highest bunching decile. To simplify the figure, we omit a plot of pre-birth earnings for individuals in the highest bunching decile, since the distribution is similar to that of the lowest bunching decile, and in particular does not exhibit any sharp bunching around the first kink of the EITC schedule. Panel B plots an event study of the fraction of individuals in the child birth sample reporting non-zero self-employment income around child birth for individuals giving birth in 1st, 5th, and 10th decile ZIP-3s.
FIGURE 9
Wage Earnings Distributions in Lowest vs. Highest Bunching Deciles

**a) Wage Earners with One Child**

**b) Wage Earners with Two or More Children**

Notes: This figure plots W-2 wage earnings distributions for households without self-employment income using data from the cross-sectional sample from 1999-2009. The series in triangles includes individuals in ZIP-3-by-year cells in the highest self-employed sharp bunching decile, while the series in circles includes individuals in the lowest sharp bunching decile. Self-employed sharp bunching is defined as the percentage of EITC claimants with children in the ZIP-3-by-year cell who report total earnings within $500 of the first EITC kink and have non-zero self-employment income. We divide the observations in the pooled dataset covering 1999-2008 into deciles of sharp bunching, so that the decile cut points remain fixed across years. Panel A plots the distribution for households with one child; panel B plots the distribution for households with two or more children in 1999-2008 and exactly two children in 2009. The figures also show the relevant EITC schedule for single households in each panel (right scale); the schedule for married households has the same first kink point but has a plateau that is extended by an amount ranging from $1,000 in 2002 to $5,000 in 2009.
FIGURE 10
Differences in Wage Earnings Distributions: Lowest vs. Highest Bunching Deciles

Notes: This figure plots the difference in the W-2 wage-earnings distributions between the highest and lowest bunching deciles. The series in circles in Panel A is the difference between the two series plotted in Figure 9a; analogously, the series in circles in Panel B is the difference between the two series plotted in Figure 9b. The series in triangles replicate the analysis of the difference in earnings distributions, restricting attention to observations in the cross-sectional analysis sample in which all of the individual’s W-2’s came from firms that filed 100 or more W-2’s in that year. The figures also show the relevant EITC schedule for single households in each panel (right scale); the schedule for married households has the same first kink point but has a plateau that is extended by an amount ranging from $1,000 in 2002 to $5,000 in 2009. See the notes to Figure 9 for further details.
FIGURE 11
Wage Earners’ EITC Amounts vs. Self-Employed Sharp Bunching Rates

Notes: This figure plots the relationship between self-employed sharp bunching rates and EITC refund amounts for wage earners (those with no self-employment income). Panel A uses the cross-sectional analysis sample from 1999-2009; Panel B uses the movers sample. In both panels, we first calculate the EITC for each household. To construct Panel A, we split the observations into 20 equal-sized bins based on the rate of self-employed sharp bunching in the ZIP-3-by-year cell. We then plot the mean EITC refund vs. the mean sharp bunching rate in each bin. The best-fit line and coefficient are derived from an OLS regression of mean EITC refund amount in each ZIP-3-by-year cell on sharp bunching rates, weighted by the number of individuals in each cell. Panel B plots the relationship between change in EITC refund and change in neighborhood sharp bunching rate for movers who are wage earners. This figure replicates Figure 6, restricting the sample to wage earners and calculating the EITC refund based on W-2 wage earnings. See the notes to Figure 6 for more details on the construction of Panel B.
FIGURE 12
Wage Earnings Distributions Before and After Birth of First Child

Notes: These figures are drawn using the child birth sample, which includes individuals from the core sample who give birth to their first child between 2000 and 2005. We classify individuals into deciles of sharp bunching based on the level of sharp bunching for residents of the ZIP-3 they inhabit in the year in which they have a child. The figures only include wage-earners (those with no self-employment income) with positive W-2 earnings. Panel A plots W-2 wage earnings distributions in the year before child birth for individuals giving birth in ZIP-3-by-year cells in the 1st, 5th, and 10th deciles. Panel B replicates these distributions for the year of child birth. The dashed lines demarcate the beginning and end of the refund-maximizing plateau region of the EITC schedule for a single individual with one child.
FIGURE 13
Event Study of Simulated EITC Around Birth of First Child

Notes: This figure plots an event study of the simulated EITC refund for wage earners around the year in which they have their first child. To calculate the simulated credit, we apply the one-child EITC schedule for single filers to total household W-2 earnings, regardless of the household’s actual structure. The figure plots mean simulated credit amounts by event year for the exactly the same three groups as in Figure 12. For scaling purposes, we normalize the level of each series at the mean simulated credit in $t = -4$; that is, we subtract the decile-specific mean in $t = -4$ and add back the mean simulated EITC across the three deciles in $t = -4$ to all observations. The coefficient compares changes in the simulated credit amount from year -1 to 0 across the highest and lowest bunching deciles, estimated using a difference-in-differences regression specification as described in the text. The standard error, reported in parentheses, is clustered at the ZIP-3-by-birth-year level. See the notes to Figure 12 for sample and bunching decile definitions.
FIGURE 14
Changes in EITC Refund Amounts Around Child Birth vs. Sharp Bunching Rates

Notes: These figures plot changes in simulated EITC refund from the year before to the year of child birth (year -1 to year 0 in Figure 13) vs. the self-employed sharp bunching rate in the individual’s ZIP-3 in the year of birth. Panel A includes only individuals in the child birth sample without self-employment income; Panel B includes all individuals in the child birth sample. In both panels we apply the one-child EITC schedule for single filers to total household W-2 earnings, regardless of the household’s actual structure and self-employment income, to calculate the simulated credit. The series in circles plots changes in simulated one-child EITC around the birth of the first child; the series in triangles plots changes in simulated one-child EITC around the birth of the third child. To construct the “0 to 1 Child” series, we split the observations with first births into twenty equal-sized bins based on the degree of self-employed sharp bunching in the individual’s ZIP-3-by-birth-year cell. Within each bin, we then calculate the mean change in simulated EITC from the year before to the year of the birth and plot this mean change against the sharp bunching rate. The “2 to 3 Child” series repeats this procedure for all third births (i.e, where the individual claimed two children the year before), once again using the one-child EITC schedule for single filers to calculate the simulated EITC credit. We estimate the best-fit lines and slopes using an OLS regression of the change in simulated credit on sharp bunching in the individual data, with standard errors clustered at the ZIP-3-by-birth-year level. See the notes to Figure 12 for further details on the child birth sample.
FIGURE 15
Phase-In, Phase-Out, and Extensive Margin Responses

a) Changes in Simulated EITC Refund Around Births

b) Extensive Margin: Changes in Fraction Working around First Birth

Notes: This figure decomposes the EITC response to the birth of a first child into the phase-in, phase-out and extensive margin responses. To do so, we replicate the “0 to 1 Child” series in Figure 14b, replacing the simulated EITC variable with other measures. Panel A distinguishes phase-in and phase-out responses. To calculate the phase-in response, we calculate the simulated credit using the schedule depicted in Appendix Figure 4a instead of the actual EITC schedule. For the phase-out response, we use the schedule depicted in Appendix Figure 4b instead. Panel B replaces the simulated EITC schedule with an indicator for positive W-2 wage earnings. We translate the extensive margin impact to an implied effect on EITC amounts by assuming that new workers earn the average EITC refund conditional on working in our sample ($1,075). The right scale in Panel B is chosen to match the scale of Panel A so that the size of the extensive margin response is scaled in the same units. The best-fit lines and standard errors are estimated as in Figure 14.
Notes: This figure plots the EITC refund and total tax refund for head-of-household filers with one dependent between 2002 and 2008. All monetary values are in 2010 dollars, indexed using the IRS inflation adjustment. The total tax refund includes the EITC and the Child Tax Credit (including the Additional Child Tax Credit) minus federal income taxes (but excluding payroll taxes). Negative values of the total tax refund indicate net tax liabilities.
APPENDIX FIGURE 2
Results with Alternative Measure of Sharp Bunching

Notes: This figure reproduces Figures 11a and 13 using an alternative definition of sharp bunching. Here, we define sharp bunching as the fraction of self-employed individuals in the ZIP-3-by-year cell who report income within $500 of the refund-maximizing kink. This definition differs from our baseline definition because we use the number of individuals with non-zero self-employment income in the denominator rather than the total number of individuals in the cross-sectional sample. In Panel A, we replace the baseline measure of sharp bunching with the alternative measure on the x-axis and reconstruct Figure 11a. To compare the coefficient in Panel A to that in Figure 11a, one must multiply the coefficient by 5.2 to account for the larger standard deviation of the alternative measure of sharp bunching. In Panel B, we define the sharp bunching deciles using the new measure and replicate Figure 13. The coefficient in Panel B can be compared directly with the coefficient in Figure 13.
APPENDIX FIGURE 3

a) Self-Employed Sharp Bunching in 1996

Notes: This figure plots sharp bunching rates by ZIP-3 in 1996. Self-employed sharp bunching is defined as the fraction of all EITC-eligible households with children in the cross-sectional sample whose total income falls within $500 of the first kink point and who have non-zero self-employment income. We divide the observations into deciles after pooling all years of the sample, so that the decile cut points remain fixed across years. Each decile is assigned a different color on the map, with darker shades representing higher levels of sharp bunching.
Notes: This figure replicates Panel A for the year 1999. Self-employed sharp bunching is defined as the fraction of all EITC-eligible households with children in the cross-sectional sample whose total income falls within $500 of the first kink point and who have non-zero self-employment income. We divide the observations into deciles after pooling all years of the sample, so that the decile cut points remain fixed across years. Each decile is assigned a different color on the map, with darker shades representing higher levels of sharp bunching.
c) Self-Employed Sharp Bunching in 2002

Notes: This figure replicates Panel A for the year 2002. Self-employed sharp bunching is defined as the fraction of all EITC-eligible households with children in the cross-sectional sample whose total income falls within $500 of the first kink point and who have non-zero self-employment income. We divide the observations into deciles after pooling all years of the sample, so that the decile cut points remain fixed across years. Each decile is assigned a different color on the map, with darker shades representing higher levels of sharp bunching.
APPENDIX FIGURE 3

d) Self-Employed Sharp Bunching in 2005

Notes: This figure replicates Panel A for the year 2005. Self-employed sharp bunching is defined as the fraction of all EITC-eligible households with children in the cross-sectional sample whose total income falls within $500 of the first kink point and who have non-zero self-employment income. We divide the observations into deciles after pooling all years of the sample, so that the decile cut points remain fixed across years. Each decile is assigned a different color on the map, with darker shades representing higher levels of sharp bunching.
APPENDIX FIGURE 3

e) Self-Employed Sharp Bunching in 2008

Notes: This figure replicates Panel A for the year 2008. Self-employed sharp bunching is defined as the fraction of all EITC-eligible households with children in the cross-sectional sample whose total income falls within $500 of the first kink point and who have non-zero self-employment income. We divide the observations into deciles after pooling all years of the sample, so that the decile cut points remain fixed across years. Each decile is assigned a different color on the map, with darker shades representing higher levels of sharp bunching.
APPENDIX FIGURE 4
Simulated Phase-In and Phase-Out Credit Schedules

Notes: This figure plots the credit schedules used to identify phase-in and phase-out responses in Figure 15 and Table III. For the phase-in schedule, the simulated credit increases from $0 to $3,050 as income rises from $0 to $8,970 (corresponding to the actual EITC phase-in schedule). The schedule is then constant at $3,050 above $8,970 in wage earnings. For the phase-out schedule, the simulated credit is constant at $3,050 for incomes up to $16,690 and then decreases to $0 at a 15.98% rate (as does the actual EITC phase-out schedule).
How can price elasticities be identified when agents face optimization frictions such as adjustment costs or inattention? I derive bounds on structural price elasticities that are a function of the observed effect of a price change on demand, the size of the price change, and the degree of frictions. The degree of frictions is measured by the utility losses agents tolerate to deviate from the frictionless optimum. The bounds imply that frictions affect intensive margin elasticities much more than extensive margin elasticities. I apply these bounds to the literature on labor supply. The utility costs of ignoring the tax changes used to identify intensive margin labor supply elasticities are typically less than 1% of earnings. As a result, small frictions can explain the differences between micro and macro elasticities, extensive and intensive margin elasticities, and other disparate findings. Pooling estimates from existing studies, I estimate a Hicksian labor supply elasticity of 0.33 on the intensive margin and 0.25 on the extensive margin after accounting for frictions.

KEYWORDS: Adjustment costs, structural estimation, partial identification.

1. INTRODUCTION

THE IDENTIFICATION OF STRUCTURAL PARAMETERS of stylized models is one of the central tasks of applied economics. Unfortunately, most models omit various frictions that make agents deviate systematically from their theoretical predictions. For instance, canonical models of labor supply or consumption behavior do not permit adjustment costs, inattentive agents, or status quo biases. How can structural parameters be identified when agents face such optimization frictions?

One natural solution is to estimate the structural parameters of a model that incorporates the frictions. This approach has two limitations in practice. First, it is difficult to incorporate all frictions in a tractable model. Second, estimating even simple dynamic models with frictions, such as $S_t$ adjustment, requires strong assumptions and is computationally challenging (Attanasio (2000)). Motivated by these limitations, I propose an alternative solution:

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bounding structural preference parameters without identifying how frictions affect behavior.

I analyze a standard dynamic life-cycle model in which the effect of income-compensated (Hicksian) price changes on demand is determined by a structural parameter of utility $\varepsilon$. I introduce optimization frictions into this nominal model through an error term in the demand function whose conditional expectation is unknown. These optimization errors generate differences between mean observed demand and the mean optimal demand predicted by the frictionless model. Because the optimization errors need not be orthogonal to the price, the observed Hicksian elasticity $\hat{\varepsilon}$ estimated from demand responses to a price change differs from the structural elasticity parameter $\varepsilon$. The observed elasticity $\hat{\varepsilon}$ confounds preferences ($\varepsilon$) with the effect of the frictions. For example, agents may underreact to a price increase in the short run because of adjustment costs.

This paper seeks to identify $\varepsilon$ from estimates of $\hat{\varepsilon}$. I focus on identifying $\varepsilon$ because it is important for both positive and normative analysis. The impacts of prices in steady state are determined purely by $\varepsilon$ in many models. Moreover, the recovery of preference parameters is essential for welfare analysis.

I bound $\varepsilon$ from observations of $\hat{\varepsilon}$ by assuming that agents choose points near the frictionless optimum. Specifically, I allow agents to deviate arbitrarily from the nominal model’s prediction as long as the expected lifetime utility cost of doing so is less than $\delta$ percent of expenditure. This property is satisfied by standard dynamic adjustment cost models, where agents remain on average within some utility threshold of their optimum. In the case of other frictions such as inattention or status quo biases, this restriction requires that agents respond to incentives that are sufficiently important.

I derive a closed-form representation for bounds on the structural Hicksian elasticity $\varepsilon$ as a function of the observed Hicksian elasticity $\hat{\varepsilon}$, the size of the price change used for identification $\Delta \log p$, and the degree of frictions $\delta$. The bounds shed light on what can be learned from reduced-form elasticity estimates in an environment with frictions. The bounds shrink at a quadratic rate with $\Delta \log p$. As a result, pooling several small price changes—although useful in improving statistical precision—yields less information about the structural elasticity than studying a few large price changes. If $\hat{\varepsilon} > 0$, the lower bound on the structural elasticity $\varepsilon$ is strictly positive, showing that frictions do not affect tests of a null hypothesis of zero response. If the observed elasticity $\hat{\varepsilon} = 0$, the upper bound on $\varepsilon$ can be expressed in terms of the utility cost of ignoring the price change. This permits straightforward calculations of the range of elasticities consistent with zero behavioral response, analogous to power calculations used to evaluate statistical precision.

The value of $\delta$ must be specified exogenously and may vary across applications. I consider the assumption that aggregate welfare would be 1% higher absent frictions ($\delta = 1\%$) to be a plausible benchmark.
The preceding results apply to an intensive margin model in which consumption is perfectly divisible. I also derive bounds on extensive margin elasticities by analyzing a model in which agents choose whether to buy an indivisible good. The bounds on the structural extensive margin elasticity ($\eta$) shrink linearly with $\delta$ and are therefore an order of magnitude tighter than those on the Hicksian intensive margin elasticity ($\varepsilon$). The bounds are tighter because the utility costs of ignoring price changes are first order on the extensive margin, in contrast with the second-order costs on the intensive margin. Hence, frictions such as adjustment costs or inattention have smaller effects on aggregate demand when microeconomic choices are discrete rather than continuous.

One can obtain tighter bounds on $\varepsilon$ or $\eta$ by calculating the least upper bound and the largest lower bound implied by multiple observed elasticities. The sensitivity of structural elasticity estimates to frictions can be evaluated by computing these unified bounds as a function of $\delta$. The smallest level of frictions $\delta_{\text{min}}$ that reconciles a group of observed elasticities provides measures of the "economic significance" of the differences in estimates. If $\delta_{\text{min}}$ is small, the differences are not economically significant in that they can be explained simply by allowing for small frictions. The value of $\varepsilon$ or $\eta$ when $\delta = \delta_{\text{min}}$ converges to the true value as the number of observed elasticities grows large, providing a point estimate of the structural elasticity adjusted for frictions.

I apply these methods to investigate what can be learned about structural labor supply elasticities from the empirical literature on labor supply. The application consists of four components, each of which addresses a different strand of the labor supply literature.

First, I analyze the impact of frictions on the intensive margin elasticity—the effects of tax changes on hours of work for employed individuals. Based on a large body of microeconometric evidence, "the profession has settled on a value for this elasticity close to zero" (Saez, Slemrod, and Giertz (2012)). I show that small frictions could explain why observed elasticities are often near zero by calculating the utility costs of ignoring tax reforms. For instance, the utility costs of ignoring the widely studied Tax Reform Act of 1986 (TRA86)—and instead choosing the optimal pre-reform level of work hours—are less than 2% of income per year for all except top income earners. Accordingly, empirical studies find that TRA86 induced behavioral responses in the short run only for top income earners. To assess what can be learned about $\varepsilon$ from existing estimates of intensive margin labor supply elasticities, I calculate bounds on $\varepsilon$ using estimates from studies of hours elasticities, taxable income elasticities, elasticities for top income earners, and macroeconomic cross-country estimates. Even though the observed elasticity estimates vary widely, all the estimates are consistent with a single structural elasticity $\varepsilon$ if one permits frictions of 1% of post-tax earnings in choosing labor supply. Pooling the 15 hours and taxable income elasticity estimates yields bounds on $\varepsilon$ of (0.28, 0.54) when
δ = 1%, with a 95% confidence interval of (0.23, 0.61). The minimum level of frictions required to reconcile these 15 estimates is δ_{min} = 0.5% of net earnings, and the corresponding point estimate of the structural Hicksian elasticity is \( \varepsilon_{\delta_{\min}} = 0.33 \).

Second, I analyze how frictions affect extensive margin elasticities—the effects of tax changes on employment rates. The utility costs of ignoring tax changes on the extensive margin are between 5 and 10% of income for many tax policy changes in the United States. These large costs could explain why microeconometric studies uniformly detect significant impacts of tax changes on employment rates despite finding negligible intensive margin responses. This result challenges the prevailing consensus that extensive margin elasticities are much larger than intensive margin elasticities. Instead, current empirical methods may simply be better suited to detect responses on the extensive margin than on the intensive margin in the presence of frictions. I calculate bounds on the structural extensive margin elasticity \( \eta \) using estimates from existing studies. The bounds on the extensive margin elasticities implied by each study are very tight, confirming that observed elasticities provide accurate estimates of structural elasticities on the extensive margin. The mean extensive margin elasticity among the microeconometric studies I consider is 0.25.

Third, I turn to the literature on nonlinear budget set (NLBS) estimation, which analyzes the impacts of progressive income taxation on labor supply. One well known issue in fitting such models is that much fewer individuals bunch at kink points of the tax system than one would predict in a frictionless model (Saez (2010)). I show that the utility gains from bunching at kinks are very small, typically less than 1% of consumption. Allowing for optimization frictions in NLBS models can explain the lack of bunching at most kinks and provide a more disciplined error structure for such models.

Finally, I assess whether frictions can explain the discrepancy between micro and macro estimates of Hicksian (steady-state) and Frisch (intertemporal substitution) elasticities. Macro estimates of the Hicksian elasticity, which are based on cross-country comparisons, are larger than micro estimates. Frictions and indivisible labor (Rogerson (1988), Rogerson and Wallenius (2009)) fully account for this gap. On the intensive margin, the micro estimate of \( \varepsilon = 0.33 \) after accounting for frictions matches cross-country evidence. On the extensive margin, micro estimates match macro cross-country estimates even without accounting for frictions, as expected given the results above. Macro estimates of the Frisch elasticity, which are based on fluctuations in labor supply over the business cycle, are also larger than micro estimates of the Frisch elasticity. To assess whether this discrepancy can be explained by frictions, I bound the structural intensive margin Frisch elasticity using the estimated structural Hicksian elasticity. The bound on the intensive Frisch elasticity is consistent with macro evidence on fluctuations in hours conditional on employment over the business cycle. However, micro estimates are not consistent with macro evidence on the extensive margin intertemporal substitution elasticity, as shown
in Chetty, Guren, Manoli, and Weber (2012). I conclude that frictions explain the gap between micro and macro estimates of steady-state elasticities relevant for cross-country comparisons, but cannot reconcile the differences between micro and macro estimates of intertemporal substitution elasticities relevant for business cycles.

The analysis here should be viewed as one step toward characterizing how frictions affect labor supply elasticities. The results are based on a standard life-cycle model of labor supply as in MaCurdy (1981) and do not account for factors incorporated into more recent models, such as human capital accumulation, credit constraints, or uncertainty. One would have to specify a nominal model that incorporates all of these structural features to bound labor supply elasticities in such an environment. This point illustrates a more general caveat: the ability to account for frictions using bounds does not provide an excuse for failing to build an accurate model. The bounds are valid only if the nominal model is correct up to optimization frictions.

This paper builds on and relates to the partial identification, near rationality, robust control, and durable goods literatures. The econometric literature on partial or set identification addresses problems such as missing data or imperfect instruments (Manski (2007), Nevo and Rosen (2008)). The present paper uses set identification to estimate structural parameters with model misspecification. I derive bounds by assuming that agents are “near rational,” as in the menu cost and near rationality literature in macroeconomics (Akerlof and Yellen (1985), Mankiw (1985), Cochrane (1989)). The focus on a class of models around a prespecified nominal model parallels the robust control literature (Hansen and Sargent (2007)). The robust control literature analyzes optimal policy with a minimax criterion and model uncertainty, whereas I consider identification of the nominal model’s parameters in the same setting. Finally, the bounds provide an alternative method of estimating preferences or production functions in models with adjustment costs. This approach requires fewer assumptions than existing methods of identifying such models (e.g., Eberly (1994), Attanasio (2000)) because it uses inputs that can be estimated using quasi-experimental techniques. However, it does not permit as rich an analysis of short-run counterfactuals because it only partially identifies the model’s parameters.

The paper is organized as follows. The next section sets up a dynamic model with frictions. The bounds on intensive and extensive margin price elasticities are derived in Section 3. Section 4 presents the application to labor supply and taxation. Section 5 concludes. Appendixes are provided in the Supplemental Material (Chetty (2012)).
2. DEMAND MODELS WITH FRICITIONS

Consider a dynamic model with \( N \) individuals who have heterogeneous tastes over two goods, \( x \) and \( y \). The price of \( x \) in period \( t \) is \( p_t \) and the price of \( y \) is fixed at 1. Individual \( i \) has wealth \( Z_i \) and chooses demand by solving

\[
\max_{x_t, y_t} \sum_{t=1}^{T} v_{i,t}(x_t, y_t) \quad \text{such that} \quad \sum_{t=1}^{T} [p_t x_t + y_t] = Z_i.
\]

To simplify exposition, I make two simplifying assumptions in the main text. First, I assume that agents face no uncertainty: prices \( p_t \) evolve deterministically. Second, I use the specification of flow utility

\[
v_{i,t}(x_t, y_t) = \begin{cases} 
  y_t + a_{i,t} \frac{x_t^{1/\epsilon}}{1 - 1/\epsilon}, & \text{if } \epsilon \neq 1, \\
  y_t + a_{i,t} \log x_t, & \text{if } \epsilon = 1. 
\end{cases}
\]

This quasilinear utility specification has three convenient properties: (i) it is a money metric, (ii) it makes the agent’s problem static because optimal demand for \( x_t \) depends only on \( p_t \) and \( a_{i,t} \), and (iii) it permits heterogeneity in demand levels but generates a constant price elasticity \( \epsilon \). I show that the main result (Proposition 1) applies to the general case where \( v_{i,t}(x_t, y_t) \) is not quasilinear and prices \( p_t \) are stochastic in Appendix A.

The utility function in (2) implies that optimal demand is

\[
x^*_i(p_t) = (\frac{a_{i,t}}{p_t})^{\epsilon}.
\]

Let \( \alpha = \sum_i \sum_t \log x^*_i(p_t = 1) / NT \) denote the mean log demand in the population when \( p_t = 1 \) and let \( \nu_{i,t} = \log x^*_t(p_t = 1) - \alpha \) denote the deviation of individual \( i \) in period \( t \) from the mean. Then we can write agent \( i \)'s demand function as

\[
\log x^*_i(p_t) = \alpha - \epsilon \log p_t + \nu_{i,t}.
\]

My objective is to identify \( \epsilon \), the structural preference parameter that controls the price elasticity of demand. More compactly, I refer to \( \epsilon \) as the “structural elasticity.” When utility is quasilinear, the Hicksian (utility constant), Marshallian (wealth constant), and Frisch (marginal utility constant) elasticities are all equal to \( \epsilon \). The bounds derived below apply to the Hicksian elasticity when utility is not quasilinear (see Appendix A). I therefore use \( \epsilon \) to denote the Hicksian elasticity in the general model in (1), in which the three elasticities differ.

Consider identification of \( \epsilon \) using a price change from \( p_A \) in period \( A \) to \( p_B \neq p_A \) in period \( B \). The standard assumption made to identify \( \epsilon \) from such variation is the following orthogonality condition on the error term \( v_{i,t} \).

\[3\] The analysis is unaffected if the identifying price variation comes from comparing two different individuals facing different prices in the same period \( A \), provided that the variation in \( p_A \) is orthogonal to the variation in tastes across individuals \( v_{i,A} \).
ASSUMPTION 1: Tastes are orthogonal to the identifying price variation: \( \mathbb{E} v_{i,A} = \mathbb{E} v_{i,B} \).

Under this assumption,

\[
\varepsilon = -\frac{\mathbb{E} \log x_{i,B}^*(p_B) - \mathbb{E} \log x_{i,A}^*(p_A)}{\log p_B - \log p_A}.
\]

Equation (3) shows that the observed response to a price change point identifies \( \varepsilon \) in the frictionless model in (1). I refer to (1) as the “nominal” model, following the robust control literature. I now show how optimization frictions affect the link between \( \varepsilon \) and the observed response using two examples.

EXAMPLE 1—Adjustment Costs: Suppose the agent must pay an adjustment cost of \( k_{i,t} \) to change his consumption of \( x \) in period \( t \). In this model, agent \( i \) chooses consumption \( x_{i,t} \) in period \( t \) by solving

\[
\max_{x_t} \sum_{t=1}^{T} \left[ a_{i,t} x_t^{1-1/\varepsilon} - p_t x_t - k_{i,t} \cdot (x_t \neq x_{t-1}) \right].
\]

Observed demand in this model, \( x_{i,t} \), differs from the frictionless optimum \( x_{i,t}^* \). Let the observed elasticity estimated from a price change between periods \( A \) and \( B \) be denoted by

\[
\hat{\varepsilon} = -\frac{\mathbb{E} \log x_{i,B}^*(p_B) - \mathbb{E} \log x_{i,A}^*(p_A)}{\log p_B - \log p_A}.
\]

In this model, \( \hat{\varepsilon} \) no longer identifies the structural elasticity \( \varepsilon \). The observed elasticity \( \hat{\varepsilon} \) may be smaller or larger than \( \varepsilon \), depending on the evolution of prices, adjustment costs, and tastes. Nevertheless, the structural elasticity \( \varepsilon \) still plays a central role in determining behavior in steady state. For example, the effects of permanent price variation across economies starting in period 1 (e.g., countries with different tax regimes) is determined purely by \( \varepsilon \). Intuitively, adjustment costs affect observed elasticities primarily in the short run, as agents delay adjustment until periods with low switching costs.

EXAMPLE 2—Price Misperceptions: A growing body of evidence indicates that individuals misperceive prices, for example, because of inattention to tax rates (DellaVigna (2009)). To model this class of deviations from (1), let \( \tilde{p}_{i,t}(p_t) \) denote agent \( i \)'s perceived price as a function of the true price in period \( t \). The agent chooses \( x_{i,t} \) by solving

\[
\max_{x_t} \sum_{t=1}^{T} \left[ a_{i,t} x_t^{1-1/\varepsilon} - \tilde{p}_{i,t}(p_t) x_t \right].
\]
The resulting observed elasticity is

\[ \hat{\varepsilon} = \varepsilon - \frac{\mathbb{E} \log \tilde{p}_{i,B}(p_B) - \mathbb{E} \log \tilde{p}_{i,A}(p_A)}{\log p_B - \log p_A}. \]

Again, the observed elasticity \( \hat{\varepsilon} \) confounds the structural elasticity of interest \( \varepsilon \) with other parameters, in this case the effect of the price change on mean perceived prices. But if perceptions converge to the truth over time, steady-state behavior is determined solely by \( \varepsilon \).

**Optimization Frictions and Partial Identification**

Examples 1 and 2 illustrate why it is challenging to accurately model and fully identify structural models with frictions. In the first example, full identification requires estimation of many primitives. The second example is more challenging because it requires specification of a theory of perceptions \( \tilde{p}_{i,t}(p_t) \). This problem motivates a less ambitious strategy: identifying \( \varepsilon \) without fully identifying the primitive sources of optimization frictions. Identifying \( \varepsilon \) is useful (though not always sufficient) for both positive and normative analysis. As discussed in the examples above, \( \varepsilon \) is sufficient to predict steady-state responses under plausible conditions. The structural elasticity \( \varepsilon \) and the observed elasticity \( \hat{\varepsilon} \) are together sufficient for welfare calculations in many applications (Chetty, Looney, and Kroft (2009)).

It is useful to recast the problem of identifying \( \varepsilon \) with unknown frictions as a partial identification problem. Define agent \( i \)'s “optimization error” as the log difference between his optimal demand under the nominal model and his observed demand: \( \phi_{i,t} = \log x^*_{i,t} - \log x_{i,t} \). Then observed demand for agent \( i \) can be written as

\[ \log x_{i,t} = \alpha - \varepsilon \log p_t + \nu_{i,t} + \phi_{i,t}. \]

Define \( x_t(p_t) = \left[ \prod_{i=1}^N x_{i,t}(p_t) \right]^{1/N} \) and \( x^*_t(p_t) = \left[ \prod_{i=1}^N x^*_{i,t}(p_t) \right]^{1/N} \) as the geometric means of observed and optimal demands.\(^5\) Mean observed (log) demand is

\[ \log x_t = \mathbb{E} \log x_{i,t} = \log x^*_t(p_t) + \mathbb{E} \phi_{i,t}. \]

\(^4\)The optimization error is an error from the econometrician’s perspective but not necessarily from the agent’s perspective. In the adjustment cost model, the agent optimizes by choosing \( x_{i,t} \) according to (4).

\(^5\)The geometric mean is analytically convenient because individuals with different levels of expenditure are weighted equally in calculations of aggregate demand elasticities. If one defines mean demand as an arithmetic mean, the results below hold if the \( \delta \) class of models in (7) is defined as requiring that the expenditure-weighted mean of utility costs is less than \( \delta \).
Unlike the preference heterogeneity error \( \nu_i \), the optimization errors \( \phi_i \) generated by frictions are not orthogonal to changes in prices. For example, in the adjustment cost model, mean observed demand may be at the optimum in period \( A \) (\( \mathbb{E}\phi_{i,A} = 0 \)), but above the new optimum following a price increase in period \( B \) (\( \mathbb{E}\phi_{i,B} > 0 \)). Without assumptions on \( \phi_i \), \( \varepsilon \) is unidentified by the observed response \( \mathbb{E}\log x_{i,B} - \mathbb{E}\log x_{i,A} \). Intuitively, if one places no restrictions on perceptions or adjustment costs, an observed response to a price change can be reconciled with any structural price elasticity.

**Restricting the Degree of Frictions**

One can obtain bounds on \( \varepsilon \) by restricting the support of \( \phi_i \) without making additional assumptions about \( \mathbb{E}\phi_i \). I restrict the support of \( \phi_i \) by requiring that agents make choices near the optimal choice under the nominal model. I obtain a money-metric measure of the utility cost of setting \( x \) suboptimally for the general nominal model in (1) using an expenditure function. Let \( U_{i,t}^* \) denote agent \( i \)'s total utility from periods \( t \) to \( T \) under his optimal consumption plan. The minimum expenditure needed to attain \( U_{i,t}^* \) when the agent sets \( x_t \) at \( \tilde{x}_t \) is

\[
e_{i,t}(\tilde{x}_t) = \min_{x_t, y_t} \sum_{s=t}^T (p_s x_s + y_s) \text{ such that } \sum_{s=t}^T v_{i,t}(x_s, y_s) \geq U_{i,t}^* \text{ and } x_t = \tilde{x}_t.
\]

The agent’s utility cost (measured in dollars) from setting \( x_{i,t} \) suboptimally is \( e_{i,t}(x_{i,t}) - e_{i,t}(x_{i,t}^*) \). I restrict the size of optimization errors by requiring that the mean utility cost as a fraction of optimal expenditure on good \( x \) is less than an exogenously specified threshold \( \delta \):

\[
\frac{1}{N} \sum_i \left[ e_{i,t}(x_{i,t}) - e_{i,t}(x_{i,t}^*) \right] / p_i x_{i,t}^* \leq \delta.
\]

The threshold \( \delta \) measures the degree of optimization frictions, scaled as a percentage of expenditure on good \( x \). For instance, \( \delta = 1\% \) permits deviations from optimal demand with an average utility cost of up to 1% of expenditure on \( x_t \).\(^6\) I measure utility costs under the nominal model because in standard models with frictions (e.g., Example 1 above), agents’ choices depend on whether

\(^6\)The appropriate choice of \( \delta \) depends on the length of time that a period represents because \( \delta \) is scaled by expenditure per period \( p_i x_{i,t}^* \). For instance, in a fixed adjustment cost model, one should set \( \delta \) to be 12 times larger when periods correspond to months rather than years.
the gains from reoptimization—as calculated under the frictionless model— exceed the size of the frictions. Because utility costs are calculated under the nominal model, the results that follow require that the nominal model is correct in a frictionless environment.

I refer to the models that generate observed demand levels \( x_{i,t} \) that satisfy (7) as a “\( \delta \) class of models” around the nominal model.\(^7\) The adjustment cost model in (4) lies in the \( \delta \) class of models around (1) if the average adjustment cost as a percentage of expenditure \( \frac{1}{N} \sum_i k_{i,t} / p_i x^*_i \leq \delta/2 \) in all periods \( t \). Intuitively, if agents face adjustment costs of less than \( \delta/2 \), they will never tolerate a utility loss of more than \( \delta \) by setting \( x_t \) suboptimally because they could always switch to \( x^*_t \) and then back to \( x_t \) in period \( t + 1 \). Similarly, the model of price misperceptions in (5) lies in the \( \delta \) class of models around (1) if the expected utility losses due to misperceptions are less than \( \delta \), that is, if perceptions are not too inaccurate on average.

Although (7) is defined based on the utility cost of setting demand suboptimally in a single period, the \( \delta \) class of models includes dynamic models in which agents make choices based on the present value of utility gains over their lifetimes. The reason is that with a suitable choice of \( \delta \), (7) provides a worst-case scenario for the choice of \( x_t \). For example, in the adjustment cost model, forward-looking agents might switch \( x_t \) to \( x^*_t \) even if the flow utility gains from doing so are smaller than \( k_{i,t}/2 \), because they can reap utility gains over their lifetimes by paying the switching cost once. However, irrespective of the path of prices and tastes, these forward-looking agents’ behavior will always satisfy (7) if \( \delta \) is specified as twice the mean adjustment cost. The choices of myopes who consider only flow utility gains will also satisfy (7). The \( \delta \) class of models thus encompasses a rich set of dynamic models of behavior around the nominal model.

A \( \delta \) class of models maps prices and primitives to a set of mean demand levels, which I denote by

\[
X_t(p_t, \delta) = \left\{ x_t : \frac{1}{N} \sum_i \left[ e_{i,t}(x_{i,t}) - e_{i,t}(x^*_i) \right] / p_t x^*_i \leq \delta \right\}.
\]

When utility is quasilinear, the choice set \( X_t(p_t, \delta) \) takes a particularly simple form. In the quasilinear case, we can assume without loss of generality that the agent splits his wealth equally across periods because the consumption path of \( y_t \) does not affect utility. Then flow utility as a function of \( x_t \) is given by

\[
u_{i,t}(x_t) = Z_t/T - p_t x_t + a_{i,t} \frac{x_t^{1-1/\epsilon}}{1 - 1/\epsilon^t}.
\]

\(^7\)The restriction on \( x_{i,t} \) in (7) is effectively a restriction on the support of the optimization error \( \phi_{i,t} \) because \( x_{i,t} = x^*_i e^{\phi_{i,t}} \).
In this case, (7) can be written as the set of demands that yield flow utility within $\delta$ units of the optimum on average:

$$X_t(p_t, \delta) = \left\{ x_t : \frac{1}{N} \sum_i [u_{i,t}(x^*_t) - u_{i,t}(x_{i,t})]/p_t x^*_t \leq \delta \right\}.$$  

Because the demand problem under the nominal model is effectively static with quasilinear utility, the lifetime utility cost of setting $x_t$ suboptimally in period $t$ is just the flow utility cost of the error. Figure 1 illustrates the construction of the choice set $X(p_t, \delta)$ with quasilinear utility when there is no heterogeneity across agents and $\delta = 1\%$. The figure plots flow utility $u(x_t)$ when $a_{i,t} = e^{3.5}$, $\epsilon = 1$, log $p_t = 1$, and $Z/T = 100$. The set of choices that yield utility within $\delta = 1\%$ of the optimum, $X(p_t, \delta) = [10.2, 14]$, is depicted by the interval on the $x$ axis.

Now consider how a price increase from $p_A$ to $p_B$ affects mean observed demand in a $\delta$ class of models. Figure 2(a) illustrates the choice sets at the two prices, $X(p_A, \delta)$ and $X(p_B, \delta)$, with the same parameters as in Figure 1. The structural elasticity $\epsilon$ controls the movement of the choice sets with the price.
FIGURE 2.—Identification with optimization frictions. Plots of the choice sets at two price levels, $X(p_A, \delta)$ and $X(p_B, \delta)$, with $\log p_A = 1$ and $\log p_B = 1.2$. In part (a), $\varepsilon = 1$; in part (b), $\varepsilon = 0$. All other parameters are specified as in Figure 1. The dashed line shows the optimal demand $x^*(p_1)$. The solid black lines in part (a) illustrate some of the responses $(\log x_B(p_B) - \log x_A(p_A))$ that may be observed for a price increase from $p_A$ to $p_B$ with a structural elasticity of $\varepsilon = 1$ and frictions of $\delta = 1\%$. 
p, as illustrated by the dashed line. The solid lines illustrate that various mean demand changes $[\log x_B(p_B) - \log x_A(p_A)]$ may be observed for a given value of $\varepsilon$. Each solid line is generated by a different model. For instance, the flat line could be generated by a model with status quo bias or satisficing consumers. Overreaction could be observed in a model with adjustment costs, for example, if there has been a history of price increases in the past. One may even observe an increase in demand, for instance, if the price increase reflects a change in tax policy that raises tax rates but makes taxes less salient.

These examples show that optimization frictions destroy the 1–1 map between the observed response and the structural elasticity in (3). Let the range of structural elasticities consistent with a given observed elasticity $\hat{\varepsilon}$ in a $\delta$ class of models be denoted by $(\varepsilon_L(\hat{\varepsilon}, \delta), \varepsilon_U(\hat{\varepsilon}, \delta))$. The objective of this paper is to characterize $\varepsilon_L$ and $\varepsilon_U$ in terms of empirically estimable parameters. The bounds $(\varepsilon_L, \varepsilon_U)$ measure the uncertainty in the structural elasticity due to potential misspecification of the behavioral model, much as a statistical confidence interval measures the uncertainty in the parameter estimate due to sampling error.$^8$

3. BOUNDS ON PRICE ELASTICITIES

I derive bounds on intensive margin elasticities in two steps. First, I characterize $X_t(p_t, \delta)$, the set of mean observed demands at a price $p_t$ for a given value of $\varepsilon$. Second, I identify the set of structural elasticities $\varepsilon$ consistent with an observed elasticity $\hat{\varepsilon}$. After establishing these results for the intensive margin case, I replicate the analysis for an extensive margin model in which $x$ is an indivisible good. Finally, I show how multiple observed elasticities can be combined to obtain more informative bounds on the structural elasticity.

Throughout, I focus on identification of bounds on $\varepsilon$, taking $\hat{\varepsilon}$ as an estimate from an infinite sample. Inference about the bounds in finite samples, where there is statistical imprecision in the estimate of $\hat{\varepsilon}$, can be handled using the techniques proposed by Imbens and Manski (2004) or Chernozhukov, Hong, and Tamer (2007).

3.1. Bounds on the Choice Set

The following lemma analytically characterizes $X_t(p, \delta)$ for small $\delta$ using a quadratic approximation to flow utility $u_{i,t}(x)$ in the quasilinear case.

**Lemma 1:** For small $\delta$, the set of mean observed demands is approximately

$$X_t(p_t, \delta) = \{x_t : |\log x_t - \log x_t^*| \leq [2\varepsilon \delta]^{1/2}\}.$$
PROOF: It is convenient to rewrite the definition of the choice set in (8) as requiring that $u_{i,t}(x_{i,t}^*) - u_{i,t}(x_{i,t}) \leq \delta_{i,t} p_t x_{i,t}^*$ and $\frac{1}{N} \sum \delta_{i,t} \leq \delta$. Here $\delta_{i,t}$ can be interpreted as the degree of frictions faced by agent $i$ in period $t$. Taking a quadratic approximation to $u_{i,t}(x) = u_{i,t}(e^{\log x})$ around $\log x_{i,t}^*$ and exploiting the first-order condition under the nominal model $u_{i,t}'(x_{i,t}^*) = 0$ yields

$$u_{i,t}(x_{i,t}^*) - u_{i,t}(x) \simeq -\frac{1}{2}(x_{i,t}^*)^2 u_{i,t}''(x_{i,t}^*)(\log x - \log x_{i,t}^*)^2. \quad (9)$$

Therefore, agent $i$’s observed demand in period $t$ must satisfy

$$|\log x_{i,t} - \log x_{i,t}^*| \leq \left[ -2\delta_{i,t} \frac{p_t}{x_{i,t}^*} \frac{1}{u_{i,t}''(x_{i,t}^*)} \right]^{1/2}. \quad (10)$$

With the quasilinear utility specification in (2), $u_{i,t}''(x_t) = \frac{\partial^2 v_{i,t}(x_t)}{\partial x^2}$ and the first-order condition in the nominal model for $x_{i,t}$ is $\frac{\partial v_{i,t}}{\partial x} (x_{i,t}(p_t)) = p_t$. Implicitly differentiating this first-order condition yields

$$u_{i,t}''(x_{i,t}^*) \frac{dx_{i,t}^*}{dp_t} = 1. \quad (11)$$

Substituting (11) into (10) gives the following restriction on demand for each agent:

$$|\log x_{i,t} - \log x_{i,t}^*| \leq [2\varepsilon \delta_{i,t}]^{1/2}. \quad (12)$$

To derive bounds on mean observed demand $x_t$, use Jensen’s inequality to obtain

$$|\log x_t - \log x_t^*| = |\mathbb{E} \log x_{i,t} - \mathbb{E} \log x_{i,t}^*| \leq \mathbb{E}[2\varepsilon \delta_{i,t}]^{1/2} \leq [2\varepsilon \delta]^{1/2}. \quad (13)$$

It follows that mean observed demand $x_t$ in a $\delta$ class of models satisfies

$$|\log x_t - \log x_t^*| \leq [2\varepsilon \delta]^{1/2}. \quad (14)$$

Note that the approximation error in this equation vanishes as $\delta \to 0$ because the remainder of the Taylor approximation in (9) involves higher order terms.

Q.E.D.
losses of deviating from the maximum of a smooth function (Akerlof and Yellen (1985), Mankiw (1985)).

Second, equation (10) shows that the width of the choice set is inversely related to the curvature of utility around the optimum, \( u''_{i,t}(x^*_{i,t}) \). A useful property of the model is that \( u''_{i,t}(x^*_{i,t}) \) is pinned down by \( \varepsilon \), the structural parameter of interest. Highly curved utilities generate small structural elasticities because the agent has a strong preference to locate near \( x^*_{i,t} \). For example, suppose the demand for an essential medicine is perfectly price inelastic at a level \( x^*_{i,t} \). The price elasticity of demand approaches zero as the curvature of the utility function approaches infinity—agents demand the medicine at any price only if they lose infinite utility by not having it. Because the utility costs of deviating from \( x^*_{i,t} \) are infinitely large, the choice set \( X_t(p_t, \delta) \) collapses to the singleton \( x^*_{t} \) for any \( \delta \) when \( \varepsilon = 0 \), as illustrated in Figure 2(b). The choice set expands as \( \varepsilon \) rises. This connection between \( \varepsilon \) and the curvature of utility is critical because it eliminates the need to estimate the additional parameter \( u''_{i,t}(x^*_{i,t}) \) when bounding \( \varepsilon \).

Finally, the set of mean observed demands depends only on the mean level of frictions \( \delta \), not on the distribution of frictions at the individual level \( \delta_{i,t} \). Because each individual’s choice set is proportional to \( [\delta_{i,t}]^{1/2} \), the potential difference between mean observed and optimal demand is greatest (the worst-case scenario) when \( \delta_{i,t} = \delta \) for all \( i, t \).

### 3.2. Bounds on the Structural Elasticity

Figure 3(a) depicts the largest structural elasticity \( \varepsilon \) that could have generated an observed elasticity \( \hat{\varepsilon} \) for a price increase from \( p_A \) to \( p_B \). When \( \varepsilon = \varepsilon_U \), mean observed demand lies at the bottom of the choice set at price \( p_A \) (log \( x_A(p_A) = \log x^*_A(p_A) - (2\varepsilon \delta)^{1/2} \)) and the top of the choice set at price \( p_B \) (log \( x_B(p_B) = \log x^*_B(p_B) + (2\varepsilon \delta)^{1/2} \)). The upper bound \( \varepsilon_U \) therefore satisfies the condition

\[
\hat{\varepsilon} = \frac{-\log x_B(p_B) - \log x_A(p_A)}{\log(p_B) - \log(p_A)}
\]

\[
= \frac{-\log x_B(p_B) - \log x_A(p_A) + 2(2\varepsilon \delta)^{1/2}}{\log(p_B) - \log(p_A)}
\]

\[
= \varepsilon_U - \frac{2(2\varepsilon_U \delta)^{1/2}}{\Delta \log p},
\]
Figure 3.—Bounding the structural elasticity with optimization frictions. The solid black line in each diagram depicts the observed demand response for a price increase from $p_A$ to $p_B$ with an observed elasticity $\hat{\varepsilon} = 0.3$, log $p_A = 1$, and log $p_B = 1.4$. Part (a) depicts the highest structural elasticity, $\varepsilon_U = 1$, that could have generated this observed response with $\delta = 1\%$. The dashed line depicts the optimal demand $x^*(p_t)$ with $\varepsilon = 1$. Part (b) analogously depicts the lowest structural elasticity, $\varepsilon_L = 0.1$, that could have generated the same observed response.
where $\Delta \log p = | \log(p_B) - \log(p_A)|$.\(^9\) Similarly, the lower bound structural elasticity $\varepsilon_L$ consistent with $\hat{\varepsilon}$, illustrated in Figure 3(b), is defined by the equation

\begin{equation}
\hat{\varepsilon} = \varepsilon_L + 2 \frac{(2\varepsilon_L \delta)^{1/2}}{\Delta \log p} .
\end{equation}

The following proposition characterizes the solutions to (12) and (13).

**PROPOSITION 1:** Under Assumption 1, for small $\delta$, the range of structural elasticities consistent with an observed elasticity $\hat{\varepsilon}$ is approximately $(\varepsilon_L, \varepsilon_U)$, where

\begin{equation}
\varepsilon_L = \hat{\varepsilon} + \frac{4\delta}{(\Delta \log p)^2} (1 - \rho) \quad \text{and} \quad \varepsilon_U = \hat{\varepsilon} + \frac{4\delta}{(\Delta \log p)^2} (1 + \rho)
\end{equation}

with

$$
\rho = \left( 1 + \frac{1}{2} \frac{\hat{\varepsilon}}{\delta} (\Delta \log p)^2 \right)^{1/2} .
$$

**PROOF:** Equations (12) and (13) both reduce to the quadratic equation $(\hat{\varepsilon} - \varepsilon)^2 = \frac{8\delta}{(\Delta \log p)^2}$. The upper and lower roots of this quadratic equation are the bounds. \(Q.E.D.\)

Equation (14) maps the magnitude of the price change $(\Delta \log p)$, the observed elasticity $\hat{\varepsilon}$, and the degree of frictions $\delta$ to bounds on the structural elasticity $\varepsilon$ when flow utility is quasilinear.\(^10\) In Appendix A, I show that when utility is not quasilinear, Proposition 1 applies to the Hicksian elasticity. In particular, if the demand function is isoelastic between $p_A$ and $p_B$, an observed Hicksian elasticity $\hat{\varepsilon}$ generates bounds on the structural Hicksian elasticity $\varepsilon$ given by exactly the same formula as (14). The discussion that follows therefore applies to Hicksian elasticities in the general model in (1).

The dashed lines in Figure 4 show the bounds $(\varepsilon_L, \varepsilon_U)$ as a function of $\hat{\varepsilon}$ with $\delta = 1\%$ of expenditure.\(^11\) Figure 4(a) considers a price change of $\Delta \log p = \ldots$

\(^9\)With $\Delta \log p$ defined as the absolute value of the log price change, the results below also apply to price reductions.

\(^10\)When $\hat{\varepsilon}$ is a finite-sample estimate, a 95% confidence set for $\varepsilon$ can be obtained by computing $\varepsilon_L$ using the lower limit of the 90% confidence interval for $\hat{\varepsilon}$ and computing $\varepsilon_U$ using the upper limit of the 90% confidence interval under certain regularity conditions (Imbens and Manski (2004)).

\(^11\)These bounds are computed using (14), which relies on a quadratic approximation to utility. To evaluate the quality of the approximation, I calculated the exact bounds with the utility in (2) numerically for a range of values of $\hat{\varepsilon} < 1$, $\Delta \log p < 100\%$, and $\delta = 1\%$. In all cases, the exact and approximate bounds differ by less than 0.001, showing that (14) is sufficiently accurate for most applications.
FIGURE 4.—Bounds on structural elasticities as a function of observed elasticities. The dashed lines show the bounds on the intensive margin structural elasticity \((\varepsilon_L, \varepsilon_U)\) versus the observed intensive margin elasticity \(\hat{\varepsilon}\), computed using Proposition 1. The dotted lines show the bounds on the extensive margin structural elasticity \((\eta_L, \eta_U)\) versus the observed extensive margin elasticity \(\hat{\eta}\), computed using Proposition 2. The solid black line is the 45 degree line. The bounds are computed with \(\delta = 1\%\) frictions and \(\Delta \log p = 20\%\) (part (a)) and \(\Delta \log p = 40\%\) (part (b)).
20%, while part (b) considers $\Delta \log p = 40\%$. The bounds offer several insights into what can be learned about structural elasticities from reduced-form estimates of observed elasticities. First, larger price changes are much more informative about $\varepsilon$ because the bounds shrink at a quadratic rate with $\Delta \log p$. With a price change of 20%, an observed elasticity of $\hat{\varepsilon} = 0.2$ is consistent with structural elasticities up to $\varepsilon_U = 2.3$. With $\Delta \log p = 40\%$ and $\hat{\varepsilon} = 0.2$, $\varepsilon_U = 0.85$. The reason for this rapid shrinkage is that the movement in the choice sets for a given value of $\varepsilon$ is greater when $\Delta \log p$ is larger, resulting in a narrower set of observed responses $\hat{\varepsilon}$ consistent with any given $\varepsilon$.

Second, the bounds are asymmetric around the observed elasticity: $\varepsilon_U - \hat{\varepsilon} > \hat{\varepsilon} - \varepsilon_L$. This asymmetry is driven by the proportional relationship between the width of the choice sets and $\varepsilon$, as shown in Lemma 1. Large structural elasticities generate wide choice sets and are therefore consistent with a broader range of $\hat{\varepsilon}$ than small structural elasticities. A related implication is that if $\varepsilon$ is small, there will be little dispersion in observed elasticities across studies, whereas a large $\varepsilon$ may lead to substantial variation in observed elasticities.

Third, the lower bound is strictly positive ($\varepsilon_L > 0$) whenever $\hat{\varepsilon} > 0$ regardless of $\delta$. If $\varepsilon = 0$, the choice sets collapse to a single point $x_i^*(p_A) = x_i^*(p_B)$ as shown in Lemma 1, and one will therefore never observe positive values of $\hat{\varepsilon}$. Agents intent on maintaining a fixed value of $x$ must face very large costs of deviating from the optimum and therefore will never do so. This result is useful for hypothesis testing: finding $\hat{\varepsilon} > 0$ is adequate to reject the null of a zero structural elasticity regardless of frictions.

Finally, consider the converse case of a study that detects zero observed behavioral response ($\hat{\varepsilon} = 0$). When $\hat{\varepsilon} = 0$, the bounds take a particularly simple form. The lower bound is $\varepsilon_L = 0$. The upper bound can be conveniently expressed in terms of the utility cost of ignoring the price change for an optimizing agent with time-invariant preferences. Consider a hypothetical agent who has fixed tastes $a_{i,t} = a_t$ across periods $A$ and $B$ and is initially at his nominal optimum $x_i^*(p_A)$. Using a quadratic approximation analogous to that in Lemma 1, this agent’s utility loss from failing to change demand to $x_i^*(p_B)$ in period $B$ is

$$
\Delta u_i \equiv u_{i,B}(x_i^*(p_B)) - u_{i,B}(x_i^*(p_A)) \\
\simeq -\frac{1}{2} u''_{i,B}(x_i^*)(\log x_i^* - \log x_i^*)^2(x_i^*)^2.
$$

---

12By the same reasoning, $\hat{\varepsilon} < 0$ implies $\varepsilon > 0$, as one could never observe a negative response if $\varepsilon = 0$. Note that negative structural elasticities ($\varepsilon < 0$) are ruled out by agent optimization in the nominal model.

13Among the feasible responses in a $\delta$ class of models, a zero response is perhaps the most likely outcome, as it requires no adjustments or attention.
Using equation (11), the utility loss from failing to reoptimize in response to a price change as a percentage of the optimal expenditure level at price $p_B$ is

$$\Delta u\%_i(\varepsilon) = \frac{\Delta u_i}{p_B x_i^*(p_B)} = \frac{1}{2}(\Delta \log p)^2 \varepsilon.$$

The utility loss $\Delta u\%_i(\varepsilon)$ is an increasing function of the structural elasticity $\varepsilon$. The following result shows that the upper bound on $\varepsilon$ when $\widehat{\varepsilon} = 0$ can be expressed in terms of $\Delta u\%_i(\varepsilon_U)$.

**COROLLARY 1:** Under Assumption 1, for a given value of $\varepsilon$, the observed elasticity $\widehat{\varepsilon}$ can be 0 only if $\Delta u\%_i(\varepsilon) \leq 4\delta$.

**PROOF:** When $\widehat{\varepsilon} = 0$, (14) implies $\varepsilon_U = 8\delta/(\Delta \log p)^2$. Combining this equation with (15) yields the result. \(Q.E.D.\)

Corollary 1 provides a simple method of determining the range of structural elasticities for which one can be sure to detect a behavioral response. Starting from the optimum, the percentage utility cost of ignoring a price change given an elasticity of $\varepsilon$ must exceed $4\delta$ to guarantee an observed elasticity $\widehat{\varepsilon} > 0$. The $4\delta$ condition is obtained because the cost of deviating from the optimum rises at a quadratic rate (see Appendix A for details). When $\widehat{\varepsilon} = 0$, $\varepsilon_U$ shrinks at a quadratic rate with $\Delta \log p$ but only at a linear rate with $\delta$. Studying a price change that is twice as large yields more information about $\varepsilon$ even if frictions are also twice as large, underscoring the value of placing greater weight on large treatments for identification.

### 3.3. Extensive Margin Elasticities

I now replicate the analysis above for the case where $x$ is an indivisible good and agents make extensive margin choices about whether to buy $x$. To analyze extensive margin responses, consider the model in (1) with the quasilinear flow utility in (2), but assume that $x \in \{0, 1\}$, so that agents make a discrete choice.

It is optimal for an agent to buy the good if its utility exceeds its price, that is, if $b_{i,t} \equiv \frac{a_{i,t}}{1+\varepsilon} > p_t$. Let the distribution of the rescaled taste parameter $b_{i,t}$ in the population be given by a smooth cumulative distribution function $F_{b_i}(b_{i,t})$ with positive support for all $b_{i,t} > 0$. I make an identification assumption analogous to Assumption 1 to ensure that elasticity estimates are unbiased without frictions.

**ASSUMPTION 1’:** Tastes are orthogonal to the identifying price variation: $F_A = F_B$. 


Let $\theta^*_t = 1 - F_t(p_t)$ denote the optimal fraction of agents who buy $x$ and let $\theta_t$ denote the observed fraction who buy $x$ in period $t$. The structural extensive margin demand elasticity for a price change from $p_A$ to $p_B$ is

$$\eta(p_A, p_B) = \frac{\log \theta^*_B(p_B) - \log \theta^*_A(p_A)}{\log(p_B) - \log(p_A)}.$$ 

The corresponding observed extensive margin elasticity is

$$\tilde{\eta}(p_A, p_B) = \frac{\log \theta_B(p_B) - \log \theta_A(p_A)}{\log(p_B) - \log(p_A)}.$$ 

Because the density $f(p)$ varies with the price, $\eta(p_A, p_B)$ varies with the price.

**ASSUMPTION 2:** The extensive margin elasticity is constant between $p_A$ and $p_B$: $\eta(p_t) = -\frac{\partial}{\partial p_t} \log \theta^*_t = \eta(p_A, p_B)$ for $p_t \in [p_A, p_B]$.

Let $\delta$ denote the degree of frictions permitted as a fraction of expenditure when buying the good, $p_t$. Then a $\delta$ class of models around the nominal extensive margin model can be defined by requiring that average utility losses are less than $\delta p_t$, as shown in Appendix A. I now establish a set of results analogous to those in the intensive margin case. The proofs of these results, which are given in the Appendix, use first-order Taylor approximations and parallel those for the intensive margin.

**LEMMA 2:** For small $\delta$, the set of participation rates is approximately

$$\Theta_t(p_t, \delta) = \{\theta_t : |\log \theta_t - \log \theta^*_t| \leq \eta(p_t)\delta\}.$$ 

The critical difference between Lemma 2 and its intensive margin analog, Lemma 1, is that the width of the choice set for participation rates is proportional to $\delta$ rather than $\delta^{1/2}$. This makes the choice set much narrower on the extensive margin than the intensive margin. With $\delta = 1\%$ and a structural elasticity of 1, the choice set spans $\pm 1\%$ of optimal aggregate demand on the extensive margin, compared with $\pm 14\%$ on the intensive margin. The choice set is much narrower because individuals incur first-order utility losses from choosing $x$ suboptimally on the extensive margin since they are not near interior optima.
The lower and upper bounds on \( \eta \) given an observed elasticity \( \hat{\eta} \) can be characterized as in Figure 3(b), leading to the following analog of Proposition 1.

**Proposition 2:** Under Assumptions 1’ and 2, for small \( \delta \), the range of structural elasticities consistent with an observed elasticity \( \hat{\eta} \) is approximately \((\eta_L, \eta_U)\), where

\[
\eta_L = \frac{\hat{\eta}}{1 + \rho_n} \quad \text{and} \quad \eta_U = \begin{cases} 
\frac{\hat{\eta}}{1 - \rho_n} & \text{if } \rho_n < 1, \\
\infty & \text{if } \rho_n \geq 1,
\end{cases}
\]

where \( \rho_n = 2\delta/\Delta \log p \).

Because the choice set grows linearly with \( \eta \) on the extensive margin, the bounds on \( \eta \) are more sensitive to the level of frictions. If the level of frictions is sufficiently large relative to the size of the identifying variation \((2\delta > \Delta \log p)\), the \( \eta \) is unbounded above because the choice sets widen more rapidly than they shift as \( \eta \) rises. Intuitively, even if no one responds to a small price change on the extensive margin, there could nevertheless be a large lurking density of agents who are very close to indifferent between buying and not buying \( x \), generating arbitrarily large \( \eta \). In contrast, on the intensive margin, we obtain a finite upper bound on \( \varepsilon \) for any price change because the choice set grows more slowly (in proportion to \((\varepsilon)^{1/2}\)) with \( \varepsilon \).

Conversely, when frictions are relatively small, the bounds on \( \eta \) are much tighter than those on \( \varepsilon \) for a given \( \delta \) because the choice set is much narrower on the extensive margin. This is illustrated by the dotted lines in Figure 4, which show the extensive margin bounds \((\eta_L, \eta_U)\) as a function of \( \hat{\eta} \). With \( \delta = 1\% \) and a price change of 20\%, an observed elasticity of \( \hat{\eta} = 0.2 \) is consistent with extensive margin structural elasticities up to \( \eta_U = 0.22 \), in contrast with the upper bound of \( \varepsilon_U = 2.3 \) for the same parameters on the intensive margin. In practice, most empirical studies generate tight bounds on \( \eta \) for plausible levels of \( \delta \), as shown in the application below. For instance, with \( \delta = 1\% \) frictions, one needs a price change of just 2\% to obtain a finite upper bound on \( \eta \).

One can also establish an analog to Corollary 1 by considering the utility cost of not responding to a price change for the agent who is just indifferent between buying and not buying at price \( p_A \), that is, the agent with \( b_i = p_A \). Let

\[
\Delta u_{\text{ext},\%} = \frac{|p_B - p_A|}{p_B} \simeq \Delta \log p
\]

denote the utility cost to this agent (as a percentage of expenditure on \( x \) when participating) of choosing \( x \) suboptimally when the price is changed to \( p_B \).\(^{14}\)

The utility cost of ignoring a price change is a first-order function of \( \Delta \log p \) on

\(^{14}\)For price cuts, the relevant utility cost is for a marginal agent who was not buying \( x \) at price \( p_A \); for price increases, the relevant utility cost is for an agent who was buying \( x \) at \( p_A \). Intuitively, the agent who experiences the largest utility cost \( \Delta u_{\text{ext},\%} \) determines the lower bound on \( \hat{\eta} \).
the extensive margin, in contrast with the second-order cost on the intensive margin in (15). Intuitively, nonparticipants enjoy the benefits of a price cut only if they reoptimize their behavior and enter the market. In contrast, on the intensive margin, the first-order increase in wealth from the price cut is automatically obtained; the benefit of reoptimization is only the second-order gain of choosing a better level of consumption.

**Corollary 2:** Under Assumptions 1’ and 2, if $\eta > 0$, then $\hat{\eta}$ can be 0 only if $\Delta u_{\text{ext}, \%} \leq 2\delta$.

If the utility cost of ignoring the price change for the marginal agent exceeds $2\delta$, we must observe $\hat{\eta} > 0$ if $\eta > 0$. Because the utility losses from ignoring price changes are first-order on the extensive margin, price changes induce behavioral responses even with substantial frictions. A 20% change in the price could produce $\hat{\eta} = 0$ only with frictions of $\delta > 10\%$ when $\eta > 0$. In contrast, the same 20% change could produce $\hat{\epsilon} = 0$ on the intensive margin with a structural elasticity of $\epsilon = 0.5$ even with $\delta = 0.25\%$. Frictions have smaller effects on aggregate demand when microeconomic choices are discrete rather than continuous because the costs of suboptimal choice are concentrated among the marginal agents with $b_{i} \simeq p_{A}$.15

### 3.4. Combining Multiple Observed Elasticities

One can obtain more information about the structural elasticity by combining multiple observed elasticities. I demonstrate this for the intensive margin, but the results that follow apply identically to extensive margin elasticities. Suppose we have a set of observed elasticities $\{\hat{\epsilon}_{1}, \ldots, \hat{\epsilon}_{J}\}$ from $J$ empirical studies. Let $\Delta \log p_{j}$ denote the size of the price change used to identify observed elasticity $j$. Let $\epsilon_{j}^{L}$ and $\epsilon_{j}^{U}$ denote the lower and upper bounds implied by study $j$, derived using Proposition 1. Let $\epsilon_{j}^{\text{max}} = \max(\epsilon_{j}^{L})$ denote the largest lower bound and let $\epsilon_{j}^{\text{min}} = \min(\epsilon_{j}^{U})$ denote the least upper bound. Then it follows that $\epsilon \in (\epsilon_{j}^{\text{max}}, \epsilon_{j}^{\text{min}})$.

By calculating $(\epsilon_{j}^{\text{max}}, \epsilon_{j}^{\text{min}})$ as a function of $\delta$, one can assess how sensitive estimates of $\epsilon$ are to frictions. One value of special interest is the smallest $\delta$ that reconciles the observed elasticities, $\delta_{\text{min}}$. When $\delta = \delta_{\text{min}}$, the structural elasticity $\epsilon$ is point identified. To characterize this minimum-$\delta$ value of $\epsilon$, let $\hat{\epsilon}_{1}$ denote the observed elasticity that produces the least upper bound and let $\hat{\epsilon}_{2}$ denote the observed elasticity that produces the highest lower bound when $\delta = \delta_{\text{min}}$. The minimum-$\delta$ estimate of $\epsilon$ satisfies $\epsilon_{\delta_{\text{min}}} = \epsilon_{\text{U}}(\hat{\epsilon}_{1}, \delta_{\text{min}}) = \epsilon_{\text{L}}(\hat{\epsilon}_{2}, \delta_{\text{min}})$.

---

15If the aggregate costs of suboptimal choice were shared across all agents, they would become a second-order function of $\Delta \log p$ because the fraction of agents who lose utility by not reoptimizing is proportional to $\Delta \log p$. 
Solving these two equations using the definitions of $\varepsilon_U$ and $\varepsilon_L$ in (12) and (13) yields the estimator

$$\varepsilon_{\delta \text{-min}} = \frac{\Delta \log p_1 \hat{\varepsilon}_1 + \Delta \log p_2 \hat{\varepsilon}_2}{\Delta \log p_1 + \Delta \log p_2}. \quad (18)$$

Equation (18) also applies to extensive margin elasticities: $\eta_{\delta \text{-min}}$ is the same weighted average of the pivotal observed elasticities.

The $\varepsilon_{\delta \text{-min}}$ estimator for the structural elasticity has two attractive features. First, it does not require exogenous specification of $\delta$. Second, if one were to observe all possible elasticities $\hat{\varepsilon}$ generated by a $\delta$ class of models, the smallest level of frictions that could reconcile the observed values of $\hat{\varepsilon}$ would be $\delta_{\text{min}} = \delta$, resulting in $\varepsilon_{\delta \text{-min}} = \varepsilon$. In this sense, $\varepsilon_{\delta \text{-min}}$ converges to $\varepsilon$ if observed elasticities are estimated in a sufficiently rich set of environments.

The value of $\delta_{\text{min}}$ can be used to formally define “economically significant” differences. If $\delta_{\text{min}}$ is small, the differences in estimates are not economically significant in that they can be reconciled simply by allowing for small frictions rather than fundamentally changing the economic model. In analogy with reporting the statistical significance of differences between estimates, the economic significance of a new estimate can be quantified by reporting the $\delta_{\text{min}}$ required to reconcile it with prior evidence.

4. APPLICATION: LABOR SUPPLY

The wage elasticity of labor supply is a parameter of central interest for tax policy analysis and macroeconomic models. A large literature in labor economics, macroeconomics, and public finance estimates this elasticity using various methods. There are many frictions that may make observed labor supply differ from optimal labor supply, such as costs of switching jobs (Altonji and Paxson (1992)), inertia (Jones (2008)), and inattention (Chetty and Saez (2009)), but few studies that estimate labor supply elasticities account for such frictions. The methods developed above are therefore well suited to extracting the information these studies contain about the structural labor supply elasticity.

I analyze the effects of frictions on four strands of the labor supply literature: intensive margin elasticities (Section 4.1), extensive margin elasticities (Section 4.2), nonlinear budget set estimation (Section 4.3), and macroeconomic elasticity estimates (Section 4.4). Throughout, I focus on identifying Hicksian elasticities relevant for steady-state comparisons. I discuss the implications of the analysis for the Frisch (intertemporal substitution) elasticity relevant for understanding business cycle fluctuations in the context of the fourth application.
4.1. Intensive Margin Elasticities

Following MaCurdy (1981), I characterize structural labor supply elasticities in a life-cycle model in which agents choose consumption \((c_t)\) and hours of work \((l_t)\) to solve

\[
\max_{c_t, l_t} \sum_{i=1}^{T} u_{i,t}(c_t, l_t) \quad \text{such that} \quad \sum_{i=1}^{T} [Y_{i,t} + (1 - \tau_t)wl_t - c_t] = 0,
\]

where \(\tau_t\) denotes the tax rate in period \(t\), \(w\) denotes the wage rate, and \(Y_{i,t}\) denotes unearned (nonwage) income. Let \(l_t^{*,*}(\tau_t)\) denote the structural Hicksonian labor supply function generated by (19). Note that (19) is equivalent to the demand model in (1) with leisure as one of the consumption goods. Because the Hicksonian wage elasticity of leisure coincides with the Hicksian wage elasticity of labor supply, Proposition A1 can be used to bound the structural labor supply elasticity

\[
\varepsilon = \log l_t^{*,*}(\tau_B) - \log l_t^{*,*}(\tau_A) \left/ \log(1 - \tau_B) - \log(1 - \tau_A) \right..
\]

In this application, \(\Delta \log p = \Delta \log(1 - \tau)\) and \(\delta\) measures frictions in choosing labor supply as a percentage of net-of-tax earnings \((1 - \tau_t)wl_t^{*,*}\).

I evaluate the impact of frictions on intensive margin elasticities in two steps. I begin by simulating the utility costs of ignoring the tax changes used for identification in the microeconometric literature. I find that the costs are typically quite small, suggesting that frictions might substantially attenuate observed elasticities (Corollary 1). I then calculate bounds on the structural Hicksonian labor supply elasticity using existing estimates of observed elasticities.

4.1.1. Utility Costs of Ignoring Tax Changes

I calculate the costs of ignoring tax changes with quasilinear, isoelastic flow utility:

\[
v_{i,t}(c_t, l_t) = c_t - a_t \frac{l_t^{1+1/\varepsilon}}{1 + 1/\varepsilon}.
\]

Let \(T_t(wl)\) denote an agent’s tax liability as a function of his taxable income in year \(t\). Since the path of consumption has no impact on the utility costs of choosing \(l\) suboptimally when utility is quasilinear, I assume without loss of generality that the agent sets consumption equal to net-of-tax income. Then flow utility as a function of the labor supply choice and tax regime is

\[
u_t(l; T_t) = wl - T_t(wl) - a_t \frac{l_t^{1+1/\varepsilon}}{1 + 1/\varepsilon}.
\]
I consider tax changes over a 3-year interval, following the convention in the literature (Gruber and Saez (2002)). Let $l^*_t$ denote optimal labor supply in period $t$ under the nominal model in (19). The utility loss in dollars from ignoring the tax changes that occur between years $t - 3$ and $t$ for an individual who sets labor supply at the optimum in year $t - 3$ is

$$\Delta u_{i,t} = u_i(l^*_t; T_t) - u_i(l^*_{t-3}; T_t).$$

I calculate $l^*_t$ and $\Delta u_{i,t}$ numerically for various values of $a_i$ and years $t$. I use a structural elasticity of $\varepsilon = 0.5$, the upper bound on $\varepsilon$ estimated below, to obtain upper bounds on utility losses. The tax rates $T_t(wl)$ are obtained from the National Bureau of Economic Research (NBER) TAXSIM calculator, including both employer and employee payroll taxes but ignoring state taxes. I consider a single tax filer with two children who has only labor income and no deductions other than those for children. I adjust for inflation in the wage $w$ using the consumer price index over the relevant 3-year period.

**Tax Reform Act of 1986.** The Tax Reform Act of 1986 (TRA86) is one of the largest reforms in the U.S. tax code and the focus of many empirical studies. Figure 5 evaluates the costs of ignoring this tax reform. Part (a) shows the marginal tax rate (MTR) schedules in 1985 (thick line) and 1988 (thin line). The dashed line, which is replicated in all the panels as a reference, shows the log change in the marginal net-of-tax rate (NTR), $\Delta \log(1 - \text{MTR})$. TRA86 increased the NTR by 15–20% for those with incomes below $100,000 and by nearly 40% for those with incomes close to $200,000.

Figure 5(b) plots the utility cost (measured in dollars) of ignoring the tax change ($\Delta u_{i,1988}$) versus gross taxable income in 1985. For instance, an individual whose taste parameter $a_i$ placed him at an optimal taxable income of $100,000 prior to TRA86 would lose $1000 by failing to reoptimize labor supply in response to the change in the tax code. Part (c) plots the cost of ignoring...
Figure 5.—The Tax Reform Act of 1986. The x axis in all the figures is gross earnings in the year prior to the reform. Part (a) shows how marginal tax rates changed between 1985 and 1988 for single filers with two children. Part (b) plots the utility cost $\Delta u_i$, measured in dollars, from failing to reoptimize labor supply on the intensive margin in response to the tax change with $\varepsilon = 0.5$. Part (c) plots the same utility cost as a percentage of net-of-tax earnings ($\Delta u_i/\%$), defined as the dollar cost in part (b) divided by the agent’s optimal net-of-tax earnings in 1988. Part (d) shows the change in gross earnings ($w_1^r,1988 - w_1^r,1985$) required to reoptimize relative to the tax change. In parts (b)–(d), the dashed line (right y axis) replicates the log change in the net-of-tax rate $(1 - MTR)$ shown in part (a).
the tax reform as a percentage of consumption, $\Delta u_{i,1988,\%} = \Delta u_{i,1988}/(wl_{i,1988} - T_{1988}(wl_{i,1988}))$. Most individuals who earn less than $100,000 lose less than 1\%$ of net earnings by ignoring TRA86 when choosing labor supply in 1988. Using Corollary 1, this result implies that frictions of $\delta = 1\%$ could lead to an
observed elasticity of \( \hat{\varepsilon} = 0 \) from TRA86 for an individual earning less than $100,000 even if his underlying structural elasticity were \( \varepsilon = 0.5 \).

Finally, part (d) plots the change in taxable income \((wl_{i,1988}^*-wl_{i,1985}^*)\) required to reoptimize relative to TRA86. With \( \varepsilon = 0.5 \), a taxpayer earning $100,000 prior to the reform would have to increase his pre-tax earnings by $13,000 to reach his new optimum. This substantial change would yield a utility gain (net of the disutility of added labor) of only $1000. Given that the search costs of immediately finding additional work that pays an extra $13,000 could well exceed $1000, it is plausible that many individuals would not respond to TRA86 within a 3-year horizon.

The costs of ignoring TRA86 are considerably larger for high-income earners. An individual earning $200,000 in 1985 would lose $4500 per year (nearly 3% of net earnings) by ignoring the tax reform. High-income individuals gain a lot more from reoptimizing both because the dollars at stake rise with income and because the change in tax rates was larger for high incomes.

Figure 6(a) extends the analysis of tax reforms to cover all tax changes from 1970 through 2006. I compute the percentage utility loss \((\Delta u_{i,t,\%})\) from ignoring tax changes at the 20th, 50th, and 99.5th percentile of the household income distribution. The value plotted for year \( t \) is the percentage utility cost of choosing \( l_{i,t-\delta}^* \) instead of \( l_{i,t}^* \) in year \( t \). There is no tax change since 1970 for which the utility cost of failing to reoptimize on the intensive margin exceeds 1% of net earnings for the median taxpayer. The utility costs of ignoring tax reforms are substantial only for the top 1% of income earners around TRA86. Correspondingly, the largest observed elasticities in historical time series are for top income earners around TRA86; for lower income groups and other time periods, observed intensive margin elasticities are near zero (Saez (2004)).

While there is little gain from adjusting behavior to optimally react to tax changes on the intensive margin over any 3-year interval, it is not the case that ignoring taxes completely imposes little cost. For example, using equation (15), the utility cost of ignoring a tax rate of \( \tau = 40\% \) and working \( l^*(\tau = 0) \) hours is \( \frac{1}{2} \cdot \frac{1}{2} \cdot (0.4)^2 = 4\% \) of net earnings per year when \( \varepsilon = 0.5 \). This is why short-run responses to tax reforms may not be very informative about how the tax system affects labor supply on the intensive margin in steady state.

20Corollary 1 applies to individuals who are at an interior optimum both before and after the tax change. In particular, a tax change could produce an observed elasticity \( \hat{\varepsilon} = 0 \) if the level of frictions \( \delta > \Delta u_{i,t,\%}(\varepsilon)/4 \) for such individuals. For individuals who optimally locate at kinks between tax brackets, the tangency conditions used to derive Corollary 1 do not hold. However, even for these agents, it is clear that a tax change could produce \( \hat{\varepsilon} = 0 \) if \( \delta > \Delta u_{i,t,\%}(\varepsilon) \).

21The total lifetime gain from reoptimizing labor supply is much larger because the agent gains $1000 every year. However, because the flow utility gains are relatively small, many agents may delay adjustment until a period where frictions (e.g., job switching costs) are lower. Thus, micro studies might not detect much change in labor supply between 1985 and 1988 even if TRA86 induced individuals to reoptimize in the long run.
FIGURE 6.—Utility cost of ignoring tax changes by year over 3-year periods from 1970 to 2006 for selected percentiles of the household income distribution. Part (a) shows the utility cost of failing to reoptimize labor supply on the intensive margin \((\Delta u_{i,t,+\%})\) with a structural intensive margin elasticity of \(\varepsilon = 0.5\), calculated as in Figure 5(c). In each year \(y\), the point that is plotted shows the utility loss (as a percentage of optimal net-of-tax earnings in year \(y\)) from choosing labor supply optimally according to the tax system in year \(y - 3\) instead of year \(y\). Part (b) depicts the percentage utility cost \((\Delta u_{i,t,ext,+\%})\) of failing to reoptimize labor supply on the extensive margin in year \(y\) for the marginal agent in year \(y - 3\). This is the agent whose disutility of working \(b_i\) made him indifferent between working and not working in \(y - 3\). The utility cost \(\Delta u_{i,t,ext,+\%}\) is measured as a percentage of net-of-tax earnings when working in year \(y\), as in Corollary 2.
4.1.2. Bounds on the Intensive Margin Hicksian Elasticity

How much can be learned about the structural Hicksian labor supply elasticity ($\varepsilon$) from existing elasticity estimates? To answer this question, I apply Proposition A1 to calculate the bounds on $\varepsilon$ implied by a set of well known studies of intensive margin labor supply. One should keep two caveats in mind when interpreting the results of the exercise. First, I assume a constant structural elasticity $\varepsilon$ across all the studies, ignoring potential variation in local preferences across tax regimes, income levels, demographic groups, or countries. Second, I assume that each study provides an unbiased estimate of the observed elasticity $\hat{\varepsilon}$. Econometric issues such as omitted variables and mean reversion may bias some of the estimates (Saez, Slemrod, and Giertz (2012)). Any such biases would pass through to the bounds.

Table I divides the studies of intensive margin labor supply into four groups: (A) studies that measure labor supply using hours of work, (B) studies that measure labor supply using taxable income, (C) studies that use taxable income but focus exclusively on top income earners, and (D) studies that rely on cross-sectional comparisons (across countries with different tax regimes or individuals with different wage rates) to estimate steady-state hours elasticities. The table lists the point estimate and the standard error of the observed Hicksian elasticity and the change in the net-of-tax rate used for identification. Details on the calculations and sources for each study are given in Appendix B. For quasi-experimental studies that analyze a single tax change, I define $\Delta \log(1 - MTR)$ as the change in the mean MTR for the treatment group (e.g., top income earners in Feldstein (1995)). For studies that pool tax or wage changes of different sizes (e.g., Gruber and Saez (2002)), I define $\Delta \log(1 - MTR)$ as twice the standard deviation of $\Delta \log(1 - MTR)$ in the sample. This is the size of the single price change that would generate the same statistical precision as the variation in $\Delta \log(1 - MTR)$ used for identification, as shown in Appendix B.

The observed elasticity estimates vary substantially across studies. Microeconometric studies of the full population find the smallest elasticities: the mean observed hours and taxable income elasticities among the studies considered in panels A and B is 0.15. Studies of top income earners find much larger elasticities, with a mean of 0.84. The mean elasticity among macroeconomic studies of steady-state responses is 0.32.

The largest observed elasticities in panels A and B are obtained from the studies that focus on the largest changes in tax policy: the abolition of the income tax for a year in Iceland (Bianchi, Gudmundsson, and Zoega (2001)) and

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22In the model in (19), the hours and taxable income elasticities are the same. I therefore pool estimates from both types of studies to bound the structural labor supply elasticity in this model. In more general models, taxable income elasticities may be larger than hours elasticities because they incorporate changes in reporting and avoidance behavior as well as changes in work effort (Slemrod (1995)).
### Table I

<table>
<thead>
<tr>
<th>Study Identification</th>
<th>( \hat{\varepsilon} )</th>
<th>s.e.(( \hat{\varepsilon} ))</th>
<th>( \Delta \log(1 - \tau) )</th>
<th>( \varepsilon_L )</th>
<th>( \varepsilon_U )</th>
<th>( \varepsilon_L )</th>
<th>( \varepsilon_U )</th>
</tr>
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<tbody>
<tr>
<td>1. MaCurdy (1981) Life-cycle wage variation, 1967–1976</td>
<td>0.15</td>
<td>0.15</td>
<td>0.39</td>
<td>0.03</td>
<td>0.80</td>
<td>0.04</td>
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<td>0.07</td>
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<td>15.29</td>
<td>0.00</td>
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<td>3. Eissa and Hoyes (1998) U.S. EITC expansions, 1984–1996, women</td>
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<td>0.07</td>
<td>0.07</td>
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<td>15.07</td>
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<td>15.30</td>
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<td>4. Blundell, Duncan, and Meghir (1998) U.K. tax reforms, 1978–1992</td>
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<td>0.09</td>
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<td>5. Ziliak and Kniesner (1999) Life-cycle wage, tax variation 1978–1987</td>
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<td>0.92</td>
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<td>0.14</td>
<td>0.14</td>
<td>0.00</td>
<td>4.42</td>
<td>0.00</td>
<td>4.84</td>
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<td>8. Saez (2004) U.S. tax reforms 1960–2000</td>
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<td>0.04</td>
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<td>13. Chetty et al. (2011) Denmark, married women, top kinks, 1994–2001</td>
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Mean observed elasticity | 0.15 |

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<th>s.c.(( \hat{\varepsilon} ))</th>
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<th>( \varepsilon_U )</th>
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<th>( \varepsilon_U )</th>
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<td>C. Top Income Elasticities</td>
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<td>17. Auten and Carroll (1999)</td>
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<td>U.S. wage variation, 1980–2000</td>
<td>0.31</td>
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<td>Mean observed elasticity</td>
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<td>Unified bounds using panels (A) and (B)</td>
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* Bounds on structural intensive margin Hicksian elasticities are shown using estimates from existing studies. Column 3 shows the point estimate of the observed elasticity, column 4 shows the associated standard error, and column 5 shows the size of the net-of-marginal-tax wage change used for identification. Columns 6 and 7 show the lower and upper bounds on the structural elasticity, calculated using Proposition A1. Columns 8 and 9 give a 95% confidence interval for \( \varepsilon \), constructed as in Imbens and Manski (2004). See Appendix B for sources and details of the underlying calculations in columns 3–5.
a Swedish tax reform in 1991 termed the “tax reform of the century” (Gelber (2010)). This pattern is consistent with the view that frictions are less likely to attenuate short-run responses to very large price changes. Excluding the Bianchi, Gudmundsson, and Zoega and Gelber studies, every point estimate in panels A and B is below all of the point estimates in panels C and D. Moreover, many of the confidence intervals for \( \hat{\varepsilon} \) in panels A and B do not overlap with the confidence intervals for \( \hat{\varepsilon} \) in panels C and D. Hence, the systematic differences in point estimates of observed elasticities across the studies in the different groups cannot be explained by statistical imprecision.

Can frictions explain the differences in the estimates? Columns 6 and 7 of Table I show the bounds \((\varepsilon_L, \varepsilon_U)\) implied by each point estimate with frictions of \( \delta = 1% \) of net earnings. Many studies that use small tax changes are consistent with structural elasticities above 1 despite obtaining small estimates. Figure 7 gives a visual representation of the bounds in columns 6 and 7. For scaling purposes, I exclude studies that use variation in net-of-tax rates of less than 20% for identification. None of the intervals plotted in the figure is disjoint; that is, all the estimates are consistent with a single structural Hicksian elasticity \( \varepsilon \) if one permits 1% frictions. Hence, the differences in estimates across high- and low-income earners as well as the differences in estimates across macroeconomic and microeconometric studies can be fully explained by small frictions.
Although any one study by itself produces wide bounds, the studies in Table I yield informative bounds on the Hicksian elasticity when combined. Intuitively, by estimating elasticities in many environments, one can obtain much sharper bounds on $\varepsilon$. The unified lower bound across the studies when $\delta = 1\%$ is $\varepsilon_L = 0.47$, obtained from Goolsbee’s (1999) analysis of TRA86. The unified upper bound is $\varepsilon_U = 0.51$, obtained from Blau and Kahn’s (2007) estimate using cross-sectional wage variation in the United States. These bounds are robust in the sense that even if one excludes these two pivotal studies, the unified bounds expand only to $(0.44, 0.54)$, with the pivotal estimates now coming from Kopczuk (2010) and Gelber (2010).

While it is instructive to demonstrate that frictions can explain the differences in estimates between panels B and C, the large elasticities for top income earners most likely reflect manipulation of reported taxable income rather than changes in labor supply (Slemrod (1995)). One may also question the validity of the estimates in panel D because of the many omitted variables and other factors that could bias cross-sectional comparisons (Alesina, Glaeser, and Sacerdote (2005)). If we only include the studies in panels A and B, the unified bounds are $(0.28, 0.54)$. These more conservative bounds are my preferred range of estimates for the structural labor supply elasticity with $\delta = 1\%$ frictions.

Figure 8 shows how the unified bounds vary with the degree of frictions. The dark shaded region shows the values of $\varepsilon$ consistent with the observed elasticities in panels A and B of Table I for $\delta \in (0, 5\%)$. The bounds widen as $\delta$ rises, but remain somewhat informative even with $\delta = 5\%$, where $\varepsilon_L = 0.15$ and $\varepsilon_U = 1.23$. Given that individuals are unlikely to tolerate utility losses equivalent to 5% of net earnings per year on average, we can rule out $\varepsilon < 0.15$ (as suggested by some microeconometric studies) or $\varepsilon > 1.23$ (as used in some macro calibrations) based on existing evidence.

The smallest value of $\delta$ that can reconcile the observed elasticity estimates in panels A and B is $\delta_{\text{min}} = 0.5\%$. That is, the differences in these 15 observed elasticity estimates are “economically significant” only if frictions in choosing labor supply are less than 0.5% of net earnings on average. The corresponding minimum-$\delta$ point estimate of the structural elasticity is $\varepsilon_{\delta_{\text{min}}} = 0.33$. This value of 0.33 is my preferred point estimate of the structural intensive margin Hicksian elasticity adjusted for frictions. Interestingly, this value is similar to the point estimates obtained from studies that are less susceptible to frictions to begin with—the steady-state cross-sectional comparisons in panel D and the micro studies of large tax changes discussed above.

Columns 8 and 9 of Table I show a 95% confidence set for the $\varepsilon$ implied by each study. These columns use the lower endpoint of the 90% confidence interval (CI) for $\hat{\varepsilon}$ to calculate $\varepsilon_L$ and the upper endpoint of the 90% CI to calculate $\varepsilon_U$ (Imbens and Manski (2004)), assuming that $\hat{\varepsilon}$ is normally distributed. In many cases, the 95% confidence sets are only slightly wider than the bounds obtained when ignoring sampling error. For instance, $\varepsilon_U$ for Gelber’s estimate
FIGURE 8.—Unified bounds on intensive margin Hicksian elasticity versus degree of frictions. This figure shows how the unified bounds on the structural intensive margin elasticity $\varepsilon$ vary with the level of frictions $\delta$. The solid lines plot the unified bounds implied by the studies in panels A and B of Table I. These unified bounds are defined only for $\delta > \delta_{\min} = 0.5\%$ because $\delta$’s below this threshold cannot reconcile the observed elasticities. The dashed lines show a 95% confidence interval for the unified bounds.

for men rises from 0.54 to 0.59. A 95% confidence set for the unified bounds can be constructed by using a simple Bonferroni bound. The 95% confidence set for the unified bounds is $(0.23, 0.61)$ when using the studies in panels A and B. These calculations indicate that the greater source of imprecision in labor supply elasticities is uncertainty about the economic model of behavior due to frictions rather than noise due to sampling error.

4.2. Extensive Margin Elasticities

I now apply the results in Section 3.3 to explain why microeconometric estimates of observed elasticities on the extensive margin are larger than those on the intensive margin (Heckman (1993)). As above, I first calculate the utility

23Given $J$ estimates $\{\hat{e}_1, \ldots, \hat{e}_J\}$, let $(\hat{e}^{CI}_L, \hat{e}^{CI}_U)$ denote a $1 - 0.05/J$ percent confidence interval for $\varepsilon$ for study $j$, calculated using the method in Imbens and Manski (2004) as above. The intersection of these $J$ regions is a (conservative) 95% CI for the unified bounds: $P[\varepsilon \in (\hat{e}^{CI}_L, \hat{e}^{CI}_U)]$ for all $j = 1 - P[\varepsilon \notin (\hat{e}^{CI}_L, \hat{e}^{CI}_U)]$ for some $j \geq 1 - \sum_{j=1}^{J} P[\varepsilon \notin (\hat{e}^{CI}_L, \hat{e}^{CI}_U)] \geq 1 - J \times 0.05/J = 0.95$. I thank Tim Armstrong for suggesting this approach.
costs of ignoring tax changes on the extensive margin and then apply Proposition 2 to bound the extensive margin Hicksian elasticity.

4.2.1. Utility Costs of Ignoring Tax Changes

I calculate the utility costs of suboptimal choice on the extensive margin using the model in (21) with \( l \in \{0, 1\} \), so that agents can only choose whether to work. I follow the same methodology as in Section 4.1.1 to calculate the utility cost of ignoring a tax change for the marginal agent in year \( t - 3 \) at each gross earnings level \( w_i \). The marginal agent at \( w_i \) has \( b_i = w_i - T_{t-3}(w_i) \). The utility cost (measured as a percentage of net-of-tax earnings when working) of choosing \( l \) suboptimally for this agent is

\[
\Delta u_{i,t,ext,\%} = \left| \log(w_i - T_t(w_i)) - \log(w_i - T_{t-3}(w_i)) \right|.
\]

Earned Income Tax Credit Expansions. Figure 9 replicates Figure 5 for another important episode in U.S. tax policy—the expansion of the Earned Income Tax Credit (EITC) under the Clinton administration. Most studies find virtually no changes in labor supply in response to EITC expansions for individuals on the intensive margin, but find a substantial response on the extensive margin (Meyer and Rosenbaum (2001), Eissa and Hoynes (2006)). Figure 9 shows that this pattern could be driven by frictions.

Figure 4(a) shows tax changes and utility costs on the intensive margin. The dashed line shows that between 1993 and 1996, net-of-tax wage rates rose by 20% for single tax filers with two children earning below $10,000 as the phase-in subsidy was increased. Meanwhile, net-of-tax wages fell by roughly 15% for those with incomes between $15,000 and $30,000 because of the increase in the phase-out tax rate. The solid curve, constructed as in Figure 5(c), shows that most individuals lose less than 1% of net earnings per year by ignoring these changes on the intensive margin. Corollary 1 implies that an observed response of \( \hat{\varepsilon} = 0 \) would be consistent with \( \varepsilon = 0.5 \) if one permits \( \delta = 1\% \) frictions in reoptimizing labor supply.

Figure 9(b) replicates part (a) for the extensive margin. The \( x \) axis of these figures is the income that the individual would earn \( (w_i) \) were he to work prior to the EITC expansion. On the extensive margin, the relevant tax rates are average rather than marginal. The dashed curve shows the change in net-of-average-tax rates (i.e., the return to working) as a result of this reform. The solid curve shows the utility cost of ignoring the EITC expansion for individuals on the margin of entering the labor force at various income levels in 1993, which coincides with the log change in the net of tax rate as shown in (17). Consider an individual who would earn $5000 when working, and is indifferent between working and not working in 1993, that is, has disutility of work \( b_i = 5000 - T_{1993}(5000) \). Figure 9(b) shows that for this marginal individual, the gain from entering the labor force in response to the Clinton EITC expansion is 18% of net income when working, which is roughly $1000. In contrast,
FIGURE 9.—Utility costs of ignoring the Clinton EITC expansion (enacted between 1993 and 1996). Part (a) considers the intensive margin. The $x$ axis is gross earnings in the year prior to the reform. The dashed line (right $y$ axis) shows the log change in the net-of-marginal-tax rate ($1 - \text{MTR}$) from 1993 to 1996 for single filers with two children. The solid line plots the utility cost as a percentage of optimal net-of-tax earnings in 1996 ($\Delta u_{i,1996}^{\text{int}} \times 100 \%$) from failing to reoptimize hours of work in response to the tax change when $\epsilon = 0.5$. Part (b) considers the extensive margin. The dashed line (right $y$ axis) shows the log change in the net-of-average-tax rate ($1 - \text{ATR}$) from 1993 to 1996 for single filers with two children. The solid line plots the utility cost ($\Delta u_{i,1996}^{\text{ext}} \times 100 \%$) of failing to enter the labor force in 1996 for the marginal agent who chose not to work at each earnings level in 1993. This is the agent whose disutility of working $b_i$ made him indifferent between working and not working in 1993 at the gross earnings level shown on the $x$ axis. The utility cost $\Delta u_{i,1996}^{\text{ext}} \times 100 \%$ is measured as a percentage of net-of-tax earnings when working in 1996.
the gain from reoptimizing hours on the intensive margin for a worker earning $5000 prior to the reform is 0.7% of income, which is roughly $50. On the extensive margin, the agent would have lost the extra $1000 EITC refund if he had ignored the tax reform and stayed out of the labor force. But on the intensive margin, a worker gets the $1000 tax reduction even if he does not change his hours. This could explain why individuals respond to the EITC expansion in the short run on the extensive margin despite frictions. Indeed, Corollary 2 implies that one could observe an elasticity of $\hat{\eta} = 0$ on the extensive margin only if frictions in adjusting labor supply exceed $\delta = 9\%$ of net-of-tax earnings when working.

Figure 6(b) extends this analysis to cover all tax changes from 1970 through 2006. In contrast to the intensive margin results shown in Figure 6(a), there are several tax changes that would generate large utility losses (5–10% of net earnings) if ignored on the extensive margin.\(^24\) The utility costs are particularly large for individuals who earn low incomes when working, which is consistent with the literature finding of the largest extensive margin responses for this group.

4.2.2. Bounds on the Extensive Margin Hicksian Elasticity

Chetty et al. (2012, Table 1) presented a meta analysis of extensive margin elasticity estimates. In Table II, I apply Proposition 2 to calculate the bounds implied by the studies they considered with $\delta = 1\%$ frictions.\(^25\) Panel A considers estimates from quasi-experimental studies, while panel B considers steady-state estimates from studies that exploit cross-sectional variation across countries or individuals. Two results emerge from this analysis.

First, the bounds on extensive margin elasticities are much tighter than those on the intensive margin, as shown in Figure 10. For instance, Eissa and Lieberman’s (1996) analysis of EITC expansions yields $\hat{\eta} = 0.30$ and bounds on $\eta$ of $(0.26, 0.36)$ with $\delta = 1\%$ frictions.\(^26\) Observed labor supply elasticities appear to provide reasonably accurate estimates of structural elasticities on the extensive margin.

Second, the heterogeneity in extensive margin elasticities across groups cannot be attributed purely to frictions. The minimum level of frictions required

\(^{24}\)In these calculations, I assume that the marginal worker is in the labor force in cases where the average tax rates rises over the 3 years and out of the labor force in cases where it falls. This is the relevant calculation to determine when one would observe zero response on the extensive margin, as shown above. I exclude the 99.5 percentile from Figure 6(b) for scaling reasons and because few individuals enter the labor force at the 99.5 percentile of the income distribution.

\(^{25}\)Among the studies considered by Chetty et al. (2012), I include only those that estimate steady-state elasticities and for which I was able to compute the size of the tax change used for identification.

\(^{26}\)The level of frictions may differ on the extensive and intensive margins. However, frictions would have to be 10 times larger on the extensive margin to generate the same impacts as on the intensive margin.
**TABLE II**

**BOUNDS ON EXTENSIVE MARGIN LABOR SUPPLY ELASTICITIES WITH $\delta = 1\%$ FRICIONS$^a$**

<table>
<thead>
<tr>
<th>Study</th>
<th>Identification</th>
<th>$\hat{\eta}$</th>
<th>s.e.($\hat{\eta}$)</th>
<th>$\Delta \log(1 - \tau)$</th>
<th>$\eta_L$</th>
<th>$\eta_U$</th>
<th>$\eta_L$</th>
<th>$\eta_U$</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
</tr>
<tr>
<td><strong>A. Quasi-Experimental Estimates</strong></td>
<td></td>
<td></td>
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<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>1. Eissa and Liebman (1996)</td>
<td>U.S. EITC expansions 1984–1990, single mothers</td>
<td>0.30</td>
<td>0.10</td>
<td>0.12</td>
<td>0.26</td>
<td>0.36</td>
<td>0.12</td>
<td>0.56</td>
</tr>
<tr>
<td>2. Graversen (1998)</td>
<td>Denmark 1987 tax reform, women</td>
<td>0.24</td>
<td>0.04</td>
<td>0.25</td>
<td>0.22</td>
<td>0.26</td>
<td>0.16</td>
<td>0.33</td>
</tr>
<tr>
<td>3. Meyer and Rosenbaum (2001)</td>
<td>U.S. welfare reforms 1985–1997, single women</td>
<td>0.43</td>
<td>0.05</td>
<td>0.45</td>
<td>0.41</td>
<td>0.45</td>
<td>0.33</td>
<td>0.53</td>
</tr>
<tr>
<td>4. Devereux (2004)</td>
<td>U.S. wage trends 1980–1990, married women</td>
<td>0.17</td>
<td>0.17</td>
<td>0.12</td>
<td>0.14</td>
<td>0.20</td>
<td>0.00</td>
<td>0.53</td>
</tr>
<tr>
<td>5. Eissa and Hoynes (2004)</td>
<td>U.S. EITC expansions 1984–1996, low-income married men and women</td>
<td>0.15</td>
<td>0.07</td>
<td>0.45</td>
<td>0.14</td>
<td>0.16</td>
<td>0.03</td>
<td>0.28</td>
</tr>
<tr>
<td>6. Liebman and Saez (2006)</td>
<td>U.S. tax reforms 1991–1997, women married to high-income men</td>
<td>0.15</td>
<td>0.30</td>
<td>0.17</td>
<td>0.13</td>
<td>0.17</td>
<td>0.00</td>
<td>0.72</td>
</tr>
<tr>
<td>7. Blundell, Bozio, and Laroque (2011)</td>
<td>U.K. tax reforms 1978–2007, prime-age men and women</td>
<td>0.30</td>
<td>n/a</td>
<td>0.74</td>
<td>0.29</td>
<td>0.31</td>
<td></td>
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<tr>
<td>Mean observed elasticity</td>
<td></td>
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<td></td>
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<td></td>
<td></td>
<td>0.25</td>
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<tr>
<td><strong>B. Macro/Cross Sectional</strong></td>
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<td></td>
<td></td>
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<tr>
<td>8. Nickell (2003)</td>
<td>Cross-country tax variation, 1961–1992</td>
<td>0.14</td>
<td>n/a</td>
<td>0.54</td>
<td>0.13</td>
<td>0.15</td>
<td></td>
<td></td>
</tr>
<tr>
<td>9. Prescott (2004)</td>
<td>Cross-country tax variation, 1970–1996</td>
<td>0.24</td>
<td>0.14</td>
<td>0.42</td>
<td>0.22</td>
<td>0.25</td>
<td>0.00</td>
<td>0.50</td>
</tr>
<tr>
<td>10. Davis and Henrekson (2005)</td>
<td>Cross-country tax variation, 1995</td>
<td>0.13</td>
<td>0.11</td>
<td>0.58</td>
<td>0.13</td>
<td>0.13</td>
<td>0.00</td>
<td>0.33</td>
</tr>
<tr>
<td>11. Blau and Kahn (2007)</td>
<td>U.S. wage variation 1989–2001, married women</td>
<td>0.45</td>
<td>0.004</td>
<td>1.00</td>
<td>0.44</td>
<td>0.45</td>
<td>0.43</td>
<td>0.46</td>
</tr>
<tr>
<td>Mean observed elasticity</td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td>0.24</td>
<td></td>
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</tbody>
</table>

$^a$Bounds on structural extensive margin elasticities are shown using estimates from existing studies. Column 3 shows the point estimate of the observed elasticity, column 4 shows the associated standard error, and column 5 shows the size of the net-of-average-tax wage change used for identification. Columns 6 and 7 show the lower and upper bounds on the structural elasticity, calculated using Proposition 2. Columns 8 and 9 give a 95% confidence interval for $\eta$, constructed as in Imbens and Manski (2004). See Appendix C for sources and details of the underlying calculations in columns 3–5.
to reconcile the extensive margin elasticities in Table II is $\delta_{\text{min}} = 18\%$. Hence, there are economically significant differences in structural extensive margin elasticities across groups. For instance, it is plausible that participation elasticities are especially large for low-income single mothers (Meyer and Rosenbaum (2001)). There may be similar heterogeneity in structural elasticities on the intensive margin, but existing evidence is inadequate to detect such heterogeneity in the presence of small frictions.

The results in Tables I and II challenge the commonly held view that extensive margin elasticities are larger than intensive margin elasticities. This view underpins some important results in modern optimal tax theory, such as providing a rationale for programs such as the Earned Income Tax Credit (Saez (2002)). The analysis here suggests that estimated intensive margin elasticities may be smaller than extensive elasticities simply because of frictions. In steady state, the intensive elasticity may actually be larger than extensive elasticities, reducing the welfare gains from programs such as the EITC.

4.3. **Bunching at Kinks and Nonlinear Budget Set Models**

The preceding two sections considered studies that analyze local changes in marginal tax rates without fully modelling each agent’s budget set in a progressive tax system. Another important strand of the literature on labor supply accounts for the entire tax system by estimating nonlinear budget set (NLBS)
models of labor supply. Frictions can also explain various patterns in the non-linear budget set literature.

(i) Bunching at Kinks. Frictionless NLBS models are rejected by the data because they predict much more bunching at kinks than observed in practice. This is illustrated in Figure 11, which plots the income tax schedule in 2006 (dashed line) for a single filer with two children. The solid grey curve shows the income distribution predicted by the frictionless model in (21) when $\varepsilon = 0.5$ and tastes $a_i$ are uniformly distributed. The frictionless model predicts sharp spikes (mass points) at each kink in a kernel density plot of the income distribution. However, empirical income distributions for wage earners exhibit no such bunching at kinks (Saez (2010)). Small frictions in choosing labor supply can explain why bunching is not more prevalent. The number next to each convex kink in Figure 11 shows the utility gain as a percentage of consumption (calculated using the utility in (21)) from locating at that kink point relative to optimizing under the incorrect assumption that the rate in the previous

![Figure 11](image-url)

Figure 11.—Gains from bunching at kinks in the 2006 tax schedule. The dashed curve shows the 2006 marginal tax rate schedule in the United States. The solid grey curve shows the distribution of taxable income predicted by the frictionless labor supply model with $\varepsilon = 0.5$. This curve assumes a uniform distribution of $a_i$ and plots an Epanechnikov kernel density of the simulated earnings distribution with a bandwidth of $1000$. The numbers near each convex kink denote the utility gain as a percentage of optimal net-of-tax earnings ($\Delta u_{nj}$) from locating at that kink when $\varepsilon = 0.5$. To compute $\Delta u_{nj}$ at a given kink, I first define $\Delta u_{nj}$ as the utility gain for an individual with taste parameter $a_i$ from locating at that kink relative to optimizing under the (incorrect) assumption that the tax rate in the previous bracket continues into the next bracket. I then define $\Delta u_{nj}$ as the unweighted mean of $\Delta u_{nj}$ over all individuals whose $a_i$ would make it optimal for them to locate at that kink. The first two kinks (1.84% and 0.71%) correspond to the end of the phase-in and start of the phase-out regions of the EITC.
bracket continues into the next bracket. The utility losses are less than 1% of net earnings at most of the kinks.

The traditional solutions used to deal with the lack of bunching at kinks when fitting NLBS models are to introduce optimization errors that smooth the income distribution around the kink (e.g., Hausman (1981), Blomquist and Hansson-Brusewitz (1990)) or to smooth the budget set itself (MacCurdy, Green, and Paarsche (1990)). The approach proposed here—permitting agents to deviate systematically from their frictionless optima provided that the utility losses fall below some threshold—places more structure on the nature of these optimization errors and could thereby improve identification.

(ii) Bunching Among the Self-Employed. Saez (2010) documented that unlike wage earners, self-employed individuals bunch at the first kink of the EITC schedule, where tax refunds are maximized. Audit studies show that self-employment income is frequently misreported on tax returns because of the lack of double reporting. Unlike changing actual hours of work, misreporting generates a first-order utility gain because it transfers resources from the government to the taxpayer. The large utility gains from misreporting taxable income could explain why the self-employed overcome frictions and bunch at this kink.

(iii) Notches. Unlike kinks, notches in budget sets, where a $1 change in earnings leads to a discontinuous jump in consumption, generate substantial behavioral responses. For example, income cutoffs to qualify for Medicaid (Yelowitz (1995)) and social security benefits in some pension systems (Gruber and Wise (1999)) induce sharp reductions in labor supply. To calculate the utility cost of ignoring a notch, suppose that earning \( w_l > K \) triggers a penalty of \( P \). Then the utility cost of setting \( l_t > K/w \) for an individual with \( l_t^* \leq K/w \) exceeds \( P \). Because the utility cost of ignoring a notch increases at a first-order rate with the size of the penalty \( P \), notches affect observed behavior substantially even with frictions. Notches are therefore a promising source of variation for identification of structural elasticities.

4.4. Micro versus Macro Elasticities

The final strand of the literature I consider is the debate on micro versus macro labor supply elasticities. Macroeconomic models calibrate labor supply elasticities to match the variation in aggregate hours of work across countries.

27There are many values of \( a_i \) that can induce individuals to locate at each kink. The numbers in the figure are (unweighted) mean percentage losses for agents who would optimally locate at the kink.

28Even the self-employed do not bunch at the second kink of the EITC schedule (where the phase-out region begins). The first kink in the EITC schedule maximizes the size of the EITC refund while minimizing payroll tax liabilities. There is no reason to locate at the second kink if one’s goal is to reap first-order gains from income manipulation.
with different tax systems or over the business cycle. In both cases, macro cal-
ibrations of representative agent models imply larger elasticities than microe-
conometric estimates of intensive margin elasticities. Can frictions explain this
gap?

The macro literature uses the term “macro elasticity” to refer to the Frisch
elasticity of aggregate hours and “micro elasticity” to refer to the intensive
margin elasticity of hours conditional on employment (e.g., Prescott (2004),
Rogerson and Wallenius (2009)). I instead use the terms “micro” and “macro”
to refer to the source of variation used to estimate the elasticity, for two rea-
sons. First, both intensive and extensive margin responses are determined by
microeconomic household-level choices. Second, the Frisch (marginal util-
ity constant) elasticity is important for understanding business cycle fluctu-
ations, but does not control the steady-state impacts of differences in taxes
across countries. The Frisch elasticity determines intertemporal substitution
responses to temporary wage fluctuations, while the Hicksian (wealth con-
stant) elasticity controls steady-state responses and the efficiency costs of taxes
(MaCurdy (1981), Auerbach (1985)). I first compare micro and macro esti-
mates of Hicksian elasticities and then turn to Frisch elasticities.

Cross-Country Evidence and Hicksian Elasticities

The mean estimate of the intensive margin Hicksian elasticity from the
two macroeconomic studies in Table I (Prescott (2004), Davis and Henrekson
(2005)) is 0.33. The mean estimate of the extensive margin Hicksian elastic-
ty from the three macroeconomic studies in Table II (Nickell (2003), Prescott
(2004), Davis and Henrekson (2005)) is 0.17. Hence, macro cross-country evi-
dence implies an aggregate hours elasticity of 0.33 + 0.17 = 0.5.

These macro elasticity estimates are consistent with micro estimates once
one accounts for optimization frictions. On the intensive margin, even the
smallest estimates in Table I are consistent with a structural elasticity of 0.33
with δ = 1% frictions. The minimum-δ micro estimate of ε = 0.33 coincides
exactly with the macro intensive elasticity. Intuitively, macroeconomic compar-
isons are more likely to overcome frictions because they analyze steady-state
behavior and because they induce coordinated changes in work patterns (Al-
tonji and Oldham (2003), Chetty, Friedman, Olsen, and Pistaferri (2011)).

On the extensive margin, the observed micro estimates in panel A of Table II
are similar to the macro and cross-sectional estimates in panel B even without
accounting for frictions. The mean micro estimate of η is 0.25. The similarity

29Chetty et al. (2012) discussed these elasticity concepts in greater detail and showed that some
discrepancies across studies arise simply from differences in terminology.
30The well known elasticity of 3 reported by Prescott (2004) is a Frisch elasticity. Regressing
log hours on log net-of-tax rates using Prescott’s data yields a Hicksian aggregate hours elasticity
of 0.7 and an intensive elasticity of 0.46. Prescott translated the Hicksian elasticity of 0.7 into a
Frisch elasticity of 3 based on specific functional form assumptions about utility.
between micro and macro estimates of extensive margin elasticities is consistent with the prediction that frictions have little impact on extensive margin responses.

I conclude that both micro and macro evidence imply steady-state aggregate hours elasticities of approximately 0.5 once one accounts for frictions and indivisible labor. Indivisible labor models show that both intensive and extensive margins are important in accounting for aggregate hours differences (Rogerson (1988), Ljungvist and Sargent (2006), Rogerson and Wallenius (2009)). Frictions explain why micro estimates of steady-state elasticities are smaller than macro estimates on the intensive margin but are similar on the extensive margin.

**Intertemporal Substitution and Frisch Elasticities**

Equilibrium macro models, in which fluctuations in labor supply are driven by preferences, require intensive margin Frisch elasticities of about 0.5 and extensive margin Frisch elasticities above 2 to fit observed fluctuations in employment and hours over the business cycle (Chetty et al. (2012)). The analysis in the present paper does not directly tell us whether micro evidence is consistent with these values because it bounds the Hicksian rather than the Frisch elasticity. Chetty et al. (2012) summarized micro estimates of the Frisch elasticity. Here, I instead show that one can obtain tight bounds on the structural intensive margin Frisch elasticity from the estimated structural Hicksian elasticity of \( \varepsilon = 0.33 \).

In the life-cycle labor supply model in (19), the intensive margin Frisch elasticity \( \varepsilon^F \) is related to the intensive margin Hicksian elasticity by the equation (Ziliak and Kniesner (1999), Browning (2005)):

\[
(22) \quad \varepsilon^F = \varepsilon + \rho \left( \frac{d[w_l^i]}{dY_{i,t}} \right)^2 \frac{A_{i,t}}{w_l^i},
\]

where \( \rho \) is the elasticity of intertemporal substitution (EIS), \( \frac{d[w_l^i]}{dY_{i,t}} \) measures the marginal propensity to earn out of unearned income (the income effect), and \( \frac{A_{i,t}}{w_l^i} \) is the ratio of assets to wage income. This equation implies \( \varepsilon^F > \varepsilon \).

One can obtain more information about \( \varepsilon^F \) by calibrating the other parameters in (22). The ratio of assets to wage earnings was approximately \( \frac{A_{i,t}}{w_l^i} = 1.26 \) for the median individual in the United States in 2008 (Dynan (2009, Table 1)). Table III shows the values of the Frisch elasticity implied by a Hicksian elasticity of \( \varepsilon = 0.33 \) and \( \frac{A_{i,t}}{w_l^i} = 1.26 \) for various combinations of \( \rho \) and \( -\frac{d[w_l^i]}{dY_{i,t}} \). To calibrate these two parameters, note that balanced growth requires that income and substitution effects cancel, implying \( -\frac{d[w_l^i]}{dY_{i,t}} = -\varepsilon \Rightarrow \frac{d[w_l^i]}{dY_{i,t}} = -0.33 \). Both micro and macro studies find an EIS of \( \rho \leq 1 \) (Hall (1988), Vissing-Jorgensen
TABLE III

<table>
<thead>
<tr>
<th>EIS (ρ)</th>
<th>0.00</th>
<th>0.11</th>
<th>0.22</th>
<th>0.33</th>
<th>0.44</th>
<th>0.55</th>
<th>0.66</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.00</td>
<td>0.33</td>
<td>0.33</td>
<td>0.33</td>
<td>0.33</td>
<td>0.33</td>
<td>0.33</td>
<td>0.33</td>
</tr>
<tr>
<td>0.20</td>
<td>0.33</td>
<td>0.34</td>
<td>0.35</td>
<td>0.36</td>
<td>0.38</td>
<td>0.41</td>
<td>0.44</td>
</tr>
<tr>
<td>0.40</td>
<td>0.33</td>
<td>0.34</td>
<td>0.36</td>
<td>0.39</td>
<td>0.43</td>
<td>0.49</td>
<td>0.55</td>
</tr>
<tr>
<td>0.60</td>
<td>0.33</td>
<td>0.34</td>
<td>0.37</td>
<td>0.42</td>
<td>0.48</td>
<td>0.56</td>
<td>0.66</td>
</tr>
<tr>
<td>0.80</td>
<td>0.33</td>
<td>0.35</td>
<td>0.38</td>
<td>0.44</td>
<td>0.53</td>
<td>0.64</td>
<td>0.77</td>
</tr>
<tr>
<td>1.00</td>
<td>0.33</td>
<td>0.35</td>
<td>0.39</td>
<td>0.47</td>
<td>0.58</td>
<td>0.71</td>
<td>0.88</td>
</tr>
<tr>
<td>1.20</td>
<td>0.33</td>
<td>0.35</td>
<td>0.41</td>
<td>0.50</td>
<td>0.63</td>
<td>0.79</td>
<td>0.99</td>
</tr>
<tr>
<td>1.40</td>
<td>0.33</td>
<td>0.35</td>
<td>0.42</td>
<td>0.53</td>
<td>0.67</td>
<td>0.87</td>
<td>1.10</td>
</tr>
</tbody>
</table>

This table shows the intensive margin Frisch elasticity implied by various combinations of the EIS and income effect. The calculations assume that the ratio of wealth to earned income is $A/w^* = 1/2$ (Dynan (2009)) and the intensive margin Hicksian (compensated) elasticity is $\varepsilon = 1/3$ (Table I). Values within the shaded area are consistent with evidence that the EIS ≤ 1 and the uncompensated labor supply elasticity is positive. The values are computed using the equation $\varepsilon_F = \varepsilon^* + \rho(d[A/w^*]/dY)^2 (A/w^*)$.

(2002), Guvenen (2006)). The largest Frisch elasticity consistent with these parameters is $\varepsilon_F = 0.47$. Intuitively, the Frisch elasticity cannot be much larger than the Hicksian elasticity for plausible values of the income effect because $\varepsilon_F - \varepsilon$ is proportional to the income effect squared and the ratio of assets to earnings is not very high for most households in the United States.

An intensive margin Frisch elasticity of 0.47 is roughly consistent with the macro evidence on business cycle fluctuations in hours of work conditional on employment. However, Chetty et al. (2012) found that fluctuations in employment rates over the business cycle imply Frisch elasticities that are an order of magnitude larger than micro estimates. Unfortunately, this discrepancy between micro and macro estimates of the Frisch elasticity on the extensive margin cannot be explained by optimization frictions.

5. CONCLUSION

There are many frictions that induce agents to deviate from the optimal choices predicted by standard economic models. This paper has shown that the model misspecification that arises from the omission of these frictions can be handled using the tools of set identification. Abstractly, I exchange the stan-

31An interesting question left for future work is whether the structural or observed elasticity is more relevant for business cycle fluctuations. If there are small fluctuations in wage rates over the business cycle, the observed elasticity (attenuated by frictions) may be the better predictor of behavioral responses. But if a small group of individuals face very large wage shocks, then frictions may be overcome and the structural elasticity may be more relevant.
standard orthogonality condition on the error term for a bounded support condition based on the utility costs of errors. I derive an analytical representation for bounds on structural price elasticities that is a function of the observed elasticity, size of the price change used for identification, and the degree of optimization frictions.

Applying the bounds to studies of taxation and labor supply offers a critique and synthesis of this literature. The critique is that many microeconometric studies of labor supply are uninformative about intensive margin elasticities because they cannot reject large values of $\varepsilon$ with frictions of even 1% of earnings in choosing labor supply. The synthesis is that several patterns in this literature can be reconciled by allowing for such small frictions. Combining estimates from several studies, my preferred point estimates of structural Hicksian elasticities are 0.33 on the intensive margin, 0.25 on the extensive margin, and roughly 0.5 for aggregate hours. I also find that Frisch elasticities cannot be much larger than Hicksian elasticities given plausible income effects.

Both the methodology and the application in this paper could be improved in several respects in future work. Methodologically, it is important to extend the bounds to settings beyond the binary treatment effect estimator considered here. Natural extensions include difference-in-difference estimates and regression models that allow for continuous price variation. It would also be interesting to explore whether the bounds can be sharpened by imposing additional restrictions from theory, such as a requirement that agents converge to unconstrained optima over time. In the labor supply application, it would be useful to calculate bounds in modern structural models of labor supply that incorporate factors such as human capital accumulation, credit constraints, and uncertainty. Because full identification of these models is challenging, bounding the structural elasticity may be a particularly fruitful approach in such cases.

Finally, the bounding methodology developed here can be applied to estimate a variety of other critical parameters such as the elasticity of intertemporal substitution, the marginal propensity to consume out of income, and the effects of the minimum wage on employment. Such analyses would shed light on which disagreements are economically significant and which can be reconciled simply by allowing for small frictions.

REFERENCES


SUPPLEMENT TO “BOUNDS ON ELASTICITIES WITH OPTIMIZATION FRICTIONS: A SYNTHESIS OF MICRO AND MACRO EVIDENCE ON LABOR SUPPLY”

(Econometrica, Vol. 80, No. 3, May 2012, 969–1018)

BY RAJ CHETTY

APPENDIX A: THEORETICAL DERIVATIONS

A.1. Bounds on Intensive Margin Elasticities With Income Effects and Stochastic Prices

This section establishes two results. First, the bounds in Proposition 1 apply to the Hicksian elasticity when the quasilinearity assumption in (2) is relaxed. Second, allowing for stochastic prices \( p_t \) does not affect the bounds. To simplify notation, I ignore heterogeneity across agents and assume all agents have a flow utility function \( v(x_t, y_t) \). Heterogeneity does not affect the result under the assumption that the structural elasticity does not vary locally across agents, as discussed below.

Let \( \mathbb{E}_t \) denote the conditional expectation operator over prices given information available in period \( t \) and let \( p = (p_1, \ldots, p_T) \) denote the realized price vector. To account for stochastic prices, I redefine the nominal model so that the agent maximizes expected lifetime utility

\[
\mathbb{E}_t \sum_{s=t}^{T} v(x_s, y_s)
\]

subject to the dynamic budget constraint \( Z_{t+1} = Z_t - p_t x_t - y_t \) and the terminal condition \( Z_{T+1} = 0 \).

Let \( V_t(p, Z_t) = \sum_{s=t}^{T} v(x_s(p), y_s(p)) \) denote the utility the agent attains from periods \( t \) to \( T \) with a realized price vector of \( p \) and wealth \( Z_t \). Following Helms (1985), I define the agent’s expenditure function with stochastic prices as the minimum wealth required to attain expected utility above a given threshold \( U \). The agent’s partial expenditure function (on all other goods) conditional on consuming \( \tilde{x}_t \) units of good \( x_t \) in period \( t \) is

\[
\tilde{e}(\tilde{x}_t, U) = \min_Z Z - p_t \tilde{x}_t \quad \text{such that} \quad \mathbb{E}_t V_t(p, Z) \geq U \quad \text{and} \quad x_t = \tilde{x}_t
\]

and hence the total expenditure function can be written as

\[
E(p_t, U) = \min_{x_t} p_t x_t + \tilde{e}(x_t).
\]
Let the expenditure-minimizing choice of \( x_i \) be denoted by \( x_i^{c,*}(p_i, U_i) \), the structural Hicksian demand function under the nominal model in (23). Let \( x_i(p_i, U_i) \) denote the observed Hicksian demand function with frictions. Let \( \varepsilon(p_i) = -\frac{\partial x_i^{c,*}}{\partial p_i} \) denote the structural Hicksian price elasticity of demand at price \( p_i \). When utility is not quasilinear, identifying \( \varepsilon(p_i) \) requires variation in prices within period \( t \) because price changes across periods confute the Frisch and Hicksian elasticities (MaCurdy (1981)). Consider an experiment in which some agents face a price of \( p_A \) and others face a price of \( p_B \) in period \( t \), and let

\[
\hat{\varepsilon}(p_A, p_B) = -\frac{\log x_B^c(p_B) - \log x_A^c(p_A)}{\log(p_B) - \log(p_A)}
\]

denote the observed elasticity from this experiment. Our objective is to identify \( \varepsilon(p_i) \) from estimates of \( \hat{\varepsilon} \) in an environment with frictions.

In this setting, the \( \delta \) class of models is defined by the condition

\[
[p_i x_i^c + \bar{c}(x_i^c)] - [p_i x_i^{c,*} + \bar{c}(x_i^{c,*})] \leq \delta p_i x_i^{c,*}.
\]

I first establish an analog of Lemma 1 to characterize the choice set with frictions.

**LEMMA A1:** For small \( \delta \), the set of observed Hicksian demands is approximately

\[
X_i^c(p_i, \delta) = \{x_i^c : |\log x_i^c - \log x_i^{c,*}| \leq |2\varepsilon(p_i)\delta|^{1/2}\}.
\]

**PROOF:** The first-order condition for (24) is

\[
\tilde{c}_x(x_i^{c,*}) = -p_i.
\]

Using a quadratic approximation to the partial expenditure function, we can exploit this first-order condition to obtain

\[
[p_i x_i^c + \bar{c}(x_i^c)] - [p_i x_i^{c,*} + \bar{c}(x_i^{c,*})] \\
\simeq \frac{1}{2}(x_i^{c,*})^2(\log x_i^c - \log x_i^{c,*})^2\tilde{c}_{xx}(x_i^{c,*})
\]

and, hence, we can rewrite (25) as

\[
|\log x_i^c - \log x_i^{c,*}| \leq \left[2\delta \frac{p_i}{x_i^{c,*}} \frac{1}{\tilde{c}_{xx}(x_i^{c,*})}\right]^{1/2}.
\]

Differentiating (27) with respect to \( p_i \) implies \( 1/\tilde{c}_{xx}(x_i^{c,*}) = -\frac{\partial x_i^{c,*}}{\partial p_i} \) and substituting this equation into (28) completes the proof. \( Q.E.D. \)
Next, I establish the analog of Proposition 1. When utility is not quasilinear, the structural elasticity $\varepsilon(p_t)$ varies with the price $p_t$. Let $\varepsilon(p_A)$ and $\varepsilon(p_B)$ denote the structural point elasticities at the initial and final prices, and let $\varepsilon(p_A, p_B) = \frac{\log x_B^c(p_B) - \log x_A^c(p_A)}{\log(p_B) - \log(p_A)}$ denote the structural arc elasticity between the two prices. Then the upper bound on $\varepsilon(p_A, p_B)$ is characterized by an equation analogous to (12):

$$\hat{\varepsilon}(p_A, p_B) = \frac{\log x_B^c(p_B) - \log x_A^c(p_A)}{\log(p_B) - \log(p_A)} = \varepsilon(p_A, p_B) - \frac{2(2\varepsilon(p_B)\delta)^{1/2}}{\Delta \log p}.$$ 

Solving this equation requires a parametric assumption about utility to relate the two point elasticities at $p_A$ and $p_B$ to the arc elasticity. I make the following local isoelasticity assumption, which is analogous to Assumption 2 in the extensive margin case.

**ASSUMPTION 2**: The structural Hicksian elasticity is constant between $p_A$ and $p_B$: $\varepsilon(p_t) = -\frac{\partial x^c}{\partial p_t} x^c = \varepsilon(p_A, p_B)$ for $p_t \in [p_A, p_B]$.

Under Assumption 2', the upper and lower bounds on the structural arc elasticity $\varepsilon(p_A, p_B)$ are characterized by the same equations as (12) and (13):

$$\hat{\varepsilon} = \varepsilon \pm \frac{2(2\varepsilon\delta)^{1/2}}{\Delta \log p}.$$ 

**PROPOSITION A1**: Under Assumption 2', for small $\delta$, the range of structural Hicksian elasticities $\varepsilon(p_A, p_B)$ consistent with an observed Hicksian elasticity $\hat{\varepsilon}(p_A, p_B)$ is approximately $(\varepsilon_L, \varepsilon_U)$, where

$$\varepsilon_L = \hat{\varepsilon} + \frac{4\delta}{(\Delta \log p)^2}(1 - \rho) \quad \text{and} \quad \varepsilon_U = \hat{\varepsilon} + \frac{4\delta}{(\Delta \log p)^2}(1 + \rho)$$

with

$$\rho = \left(1 + \frac{1}{2} \frac{\hat{\varepsilon}(p_t)}{\delta}(\Delta \log p)^2\right)^{1/2}.$$ 

The proof is identical to the proof of Proposition 1.

In a model with heterogeneous utilities $v_i(x, y_i)$, Proposition A1 requires a stronger isoelasticity assumption, namely that the structural elasticity $\varepsilon(p_t)$ does not vary across agents between $p_A$ and $p_B$. It also requires an assump-
tion analogous to Assumption 1, that is, that tastes are orthogonal to the price change used for identification.

A.2. Bounds on Extensive Margin Elasticities

With quasilinear utility, the agent’s flow utility in period $t$ is $v_{i,t}(x, y) = y + b_{i,t}x$. Recognizing that the consumption path of $y$ does not affect lifetime utility, the flow utility cost of choosing $x$ suboptimally in period $t$ is

$$u_{i,t}(x^*(p_t)) - u_{i,t}(x) = (x^*_i - x)(b_{i,t} - p_t).$$

I define a $\delta$ class of models around the nominal model by a condition analogous to (7):

$$x^*_i - x(b_{i,t} - p_t) \leq \delta_i p_t \quad \text{and} \quad \frac{1}{N} \sum_i \delta_{i,t} \leq \delta \quad \text{and}$$

$$F(b_{i,t}|\delta_{i,t}) = F(b_{i,t}).$$

The last condition in (29)—that the taste distribution cannot vary across agents with different frictions—is needed to ensure that the choice set has the same width for the marginal agents at each level of $p$.\footnote{To see why this condition is needed, suppose agents with $b_{i,t}$ close to $p_t$ have very large $\delta_{i,t}$ while those away from the margin have $\delta_{i,t} = 0$. This would result in a wide choice set for the participation rate at $p_t$ even if $\mathbb{E}\delta_{i,t} < \delta$.} This condition was not necessary in the intensive margin case because there the marginal agent did not vary with $p$.

**PROOF OF LEMMA 2:** Equation (29) implies that agent $i$’s observed demand for $x$ is

$$x_{i,t} = \begin{cases} 1, & \text{if } b_{i,t} - p_t > \delta_{i,t} p_t, \\ \{0, 1\}, & \text{if } |b_{i,t} - p_t| \leq \delta_{i,t} p_t, \\ 0, & \text{if } b_{i,t} - p_t < -\delta_{i,t} p_t. \end{cases}$$

Let $\theta_{\delta_{i,t}}(p_t)$ denote the observed participation rate for agents who have frictions $\delta_{i,t}$ and let $\theta_t = \mathbb{E}\theta_{\delta_{i,t}}(p_t)$ denote the observed participation rate in the aggregate economy. Under the condition that $F(b_{i,t}|\delta_{i,t}) = F(b_{i,t})$, it follows that $\theta_{\delta_{i,t}}(p_t)$ lies in the set

$$[1 - F((1 + \delta_{i,t}) p_t), 1 - F((1 - \delta_{i,t}) p_t)]$$

$$= \left[ \theta^*_t + F(p_t) - F((1 + \delta_{i,t}) p_t), \theta^*_t + F(p_t) - F((1 - \delta_{i,t}) p_t) \right]$$

$$\simeq \left[ \theta^*_t - f(p_t)p_t\delta_{i,t}, \theta^*_t + f(p_t)p_t\delta_{i,t} \right].$$
where the last line uses a first-order Taylor expansion of $F(p_t)$ around $p_t$. Under Assumptions 1$'$ and 2$'$, $\eta = -\frac{d \log(1-F(p_t))}{d \log p_t} \approx \frac{f(p_t)}{f'(p_t)} p_t$. Hence

$\theta_{\tilde{t}, t}(p_t) \in [\theta^*_t \cdot (1 - \eta \delta_{t, t}), \theta^*_t \cdot (1 + \eta \delta_{t, t})]$

$\Rightarrow \mathbb{E} \theta_{\tilde{t}, t}(p_t) \in [\theta^*_t \cdot (1 - \eta \mathbb{E} \delta_{t, t}), \theta^*_t \cdot (1 + \eta \mathbb{E} \delta_{t, t})]$

$\Rightarrow \theta_t(p_t)/\theta^*_t(p_t) \in [1 - \eta \delta, 1 + \eta \delta]$.

The approximation $\log(1 + \eta \delta) \approx \eta \delta$ for small $\delta$ yields $|\log \theta_t - \log \theta^*_t| \leq \eta \delta$.

**Q.E.D.**

**PROOF OF PROPOSITION 2:** Given a structural elasticity $\eta$, the maximal observed response to a price change of $\Delta \log p$ is $\Delta \log \theta = \eta \Delta \log p + 2 \delta \eta$ and the minimal observed response is $\Delta \log \theta = \eta \Delta \log p - 2 \delta \eta$. Therefore, the observed elasticity $\hat{\eta} = \frac{\Delta \log \theta}{\Delta \log p}$ must satisfy

$$(30) \quad (1 - \rho_\eta) \eta \leq \hat{\eta} \leq (1 + \rho_\eta) \eta,$$

where $\rho_\eta = \frac{2 \delta}{\Delta \log p}$. If $\rho_\eta \geq 1$, $\eta$ is unbounded above for a given value of $\hat{\eta}$ because both inequalities in (30) are satisfied for arbitrarily large $\eta$. If $\frac{2 \delta}{\Delta \log p} < 1$, then the upper and lower bounds on $\eta$ are obtained when (30) holds with equality. Solving these equations yields (16).

**Q.E.D.**

**PROOF OF COROLLARY 2:** Suppose $\hat{\eta} = 0$. Then $\rho_\eta < 1 \Rightarrow \eta_U = 0$. Hence a positive structural elasticity ($\eta > 0$) can only generate a 0 observed elasticity if $\rho_\eta = \frac{2 \delta}{\Delta \log p} \geq 1 \Leftrightarrow \Delta u_{ext, \%} = \Delta \log p \leq 2 \delta$.

**Q.E.D.**

A.3. Intuition for 4$\delta$ Threshold in Corollary 1

This section explains why $\Delta u_{\%}(\epsilon)$ must be below 4$\delta$ so as to observe $\hat{\epsilon} = 0$. Let $d = x^*_A(p_A) - \min(X_A(p_A, \delta))$ denote the difference between the mean optimal demand and the lowest mean demand in the initial choice set. Figure 1(a) shows that at the upper bound $\epsilon_U$, the difference between the optimal demands at the two prices is $x^*(p_A) - x^*(p_B) = 2d$. By definition, the percentage utility cost of choosing $\min(X_A(p_A, \delta))$ instead of $x^*(p_A)$ is $\delta$. Given that the utility cost of deviating by $d$ units is $\delta$, the utility cost of deviating by $2d$ units is $4\delta$, as illustrated in Figure 1(b).

**APPENDIX B: SOURCES AND CALCULATIONS FOR STUDIES IN TABLE I**

This appendix describes how the values in columns 3–5 in Table I are calculated. The papers used for the analysis along with comprehensive documentation of the calculations are available at http://obs.rc.fas.harvard.edu/chetty/bounds_opt_meta_analysis.zip.
I use compensated intensive margin estimates reported in each paper when available and use the Slutsky equation to calculate compensated elasticities in cases where uncompensated elasticities are reported.

The studies do not always directly report the relevant inputs, especially the net-of-tax change $\Delta \log(1-\tau)$. For studies whose estimates are identified from a single quasi-experiment (e.g., Feldstein (1995)), I define $\Delta \log(1-\tau)$ as the change in the marginal NTR for the group that the authors’ define as the “treated” group. For studies that pool multiple tax or wage changes of different sizes and do not explicitly isolate a treatment group (e.g., Gruber and Saez (2002)), I define $\Delta \log(1-\tau)$ as twice the standard deviation (SD) of $\Delta \log(1-\text{MTR})$ in the sample. The logic for this approach is as follows. In a linear regression $Y_i = \alpha + \beta_1 X_i + u_i$, the standard error of $\hat{\beta}_1$ is the square root of $\frac{\text{var}(u)}{\text{var}(X)}/N$, where $N$ is the sample size. Consider a second regression $Y_i = \alpha + \beta_2 Z_i + u_i$, where $Z_i = 0$ for half the observations (the “control group”) and $Z_i = 2 \cdot \text{SD}(X)$ for the remaining observations (the “treatment group”). Setting the size of the single treatment to $2 \cdot \text{SD}(X)$ yields $\text{var}(Z) = \text{var}(X)$. Hence, the standard error of $\hat{\beta}_2$ equals the standard error of $\hat{\beta}_1$. A single tax change of $2 \cdot \text{SD}(\Delta \log(1-\text{MTR}))$ therefore produces an estimate of $\hat{\varepsilon}$ with the same precision as the original variation in marginal tax rates used for identification.

I calculate the bounds by assuming that agents face a linear budget set whose slope is given by their marginal tax rate (MTR) and apply Proposition A1 using $\Delta \log(1-\text{MTR})$ in place of $\Delta \log p$. This yields valid bounds on $\varepsilon$ for agents who remain in the interior of budget segments in a progressive tax system. However, the bounds cannot be applied to agents who locate at kinks. Given
that most of the studies in Table I estimate elasticities from changes in the behavior of agents away from kinks, this is not a serious limitation.33 The remainder of this appendix describes how I calculate $\hat{\varepsilon}$, the standard error of $\hat{\varepsilon}$, and $\Delta \log(1 - MTR)$ for each study in Table I.

### A. Hours Elasticities

1. MaCurdy (1981). $\hat{\varepsilon}$ is reported in the text on page 1083; s.e.$(\hat{\varepsilon})$ is imputed from the $t$-statistic for $\delta$ reported in row 5 of Table 1 as $0.15/0.98$, because the estimate of compensated elasticity is approximately equal to $\delta$; $\Delta \log(1 - \tau)$, the relevant within-person annual wage variation, is not reported in the paper, so I use $2 \times \text{SD} = 2 \times (0.152^2 + 2 \cdot 0.086^2)^{1/2}$ from Table 1, column 4 of Low, Meghir, and Pistaferri (2010), who estimated the standard deviation of changes in log wages. Note that this is likely an overestimate of the size of $\Delta \log(1 - \tau)$, resulting in bounds that are too tight, because MaCurdy used family background characteristics, age, and year dummies as instruments for wage growth and did not use all elements of wage growth for identification.

2, 3. Eissa and Hoynes (1998). $\hat{\varepsilon}$ is reported for men as an intensive margin “wage elasticity” of $0.07$ and an income elasticity of $-0.03$ in Table 8, column 3. This “wage elasticity” uses the total hours change, which includes the hours change induced by the increased EITC rebate, which raised the average net of tax rate by $0.042$ for a couple earning $15,000$ with two children (for whom the average net-of-tax rate changed from $107.5\%$ in 1993 to $112.1\%$ in 1994 computed using TAXSIM). This rebate should have changed hours (in log terms) by $-0.03 \times 0.042$, giving an uncompensated elasticity of $0.069$. The compensated elasticity is $\hat{\varepsilon}_{l,w} = \hat{\varepsilon}_{l,y} - \hat{\varepsilon}_{l,y} = 0.200$, with $w, l, y$ from Table 3, column 4. A parallel calculation using Table 9 gives $\hat{\varepsilon}_{l,w} = 0.888$. The s.e.$(\hat{\varepsilon})$ assumed that $w, l, y$, and the change in income from the EITC expansion are measured without error. Then using the $t$-statistics from the coefficients on $\ln(\text{wage})$ and virtual inc to impute the standard errors for the elasticities yields $\text{SE}(\hat{\varepsilon}_{l,w}) = \{\text{SE}(\hat{\varepsilon}_{l,w})^2 + \frac{w}{y} \text{SE}(\hat{\varepsilon}_{l,y})^2\}^{1/2} = 0.074$ and $\text{SE}(\hat{\varepsilon}_{l,w}^\text{men}) = 0.067$. Note that this calculation is limited because the full variance–covariance matrix for the regression coefficients is not reported. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD of log net-of-tax-rate in the phase-out EITC rates listed in Table 1 for 1984–1996, because most married couples who receive the EITC are in the phase-out region (Table 2).}

33Recent studies that identify observed elasticities from bunching at kinks (e.g., Saez (2010), Chetty, Friedman, Olsen, and Pistaferri (2011b)) are an exception. I incorporate these studies into the linear-demand framework by exploiting the fact that they also study movements in the kinks over time, which create reductions in marginal rates for the subgroup of individuals located between the old and new bracket cutoffs. These studies imply that these individuals do not increase labor supply significantly when their marginal tax rates are lowered. This constitutes an observed elasticity estimate based on choices at interior optima, permitting application of Proposition 1.
4. Blundell, Duncan, and Meghir (1998). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are from Table 4, row 1. I interpret this estimate as an intensive margin elasticity because the variation in wages from the grouping estimator does not appear to affect participation, based on the discussion on page 845. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD}(\log \hat{w}_{gt} - \log \hat{w}_g - \log \hat{w}_t) = 0.23$, which is reported in Table 9, because the variation arises from group–time interactions in wages.

5. Ziliak and Kniesner (1999). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are from Table 1, column 3. $\Delta \log(1 - \tau)$ is the study that effectively uses within-person annual wage variation, because lagged wage growth is included as an instrument. Since within-person annual wage variation is not reported in the paper, I again use $2 \times \text{SD} = 2 \times (0.152^2 + 2 \cdot 0.086^2)^{1/2}$ from Table 1, column 4 of Low, Meghir, and Pistaferri (2010).

B. Taxable Income Elasticities

6. Bianchi, Gudmundsson, and Zoega (2001). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are the average percent change in earnings for men and women weighted by observations (columns 1–4 of Table 6) divided by the percent change in the net-of-tax rate. The standard error is computed from the standard errors reported for the changes in earnings. I interpret this estimate as an intensive margin elasticity because Table 6 conditions on work in 1986, and tax rates were generally lower in 1987 and 1988 than in 1986. I take this to be a compensated elasticity because Bianchi, Gudmundsson, and Zoega argued that income effects are small on page 1565–1566, although this is somewhat tenuous. Note that the elasticity estimates provided by the authors are computed using average rather than marginal tax rates, necessitating the use of the computation described above. $\Delta \log(1 - \tau)$ is the log change from a tax rate of 0 in 1987 to 0.3875, which is an average of the flat tax in 1988 and the mean of the top marginal tax rate and bottom marginal tax rate in 1986 reported in Table 1, because the change in earnings estimate compares 1987 to the average earnings in 1986 and 1988.

7. Gruber and Saez (2002). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are averages of the estimates in column 2 of Table 9 for individuals with taxable income between $10,000 and $50,000 and those with taxable income between $50,000 and $100,000. These estimates are compensated elasticities, as Gruber and Saez note on page 20 that income effects are essentially zero in their sample. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD}$ of the change in log net-of-tax-rate and is computed separately for columns 3 and 4 of Table 3 using the means and standard deviations for each year. The two estimates of $\Delta \log(1 - \tau)$ are then averaged in the same way as in the elasticity calculation described above.

8. Saez (2004). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are from Table 7B, column 6 for the top 5% to 1% of tax units. Note that Saez used gross income, not taxable income. I interpret his estimate as an intensive margin elasticity because his sample consists of repeated cross sections of workers and because the extensive margin is unlikely to be important for the top 5% to 1% of taxpayers. I interpret this
estimate as a compensated elasticity following the aforementioned evidence from Gruber and Saez (2002) that income effects are small. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD of the log net-of-tax-rate for the top 5% to 1% of tax units listed in column 8 of Table 5.}

9. Jacob and Ludwig (2008). For $\hat{\varepsilon}$, these authors report in Table 3 that head of households’ quarterly earnings conditional on working changed by $228 from a control mean of $5558. As with Eissa and Hoynes, I calculate how much income would have changed absent the grant worth $6860 (page 9) so as to compute a compensated wage elasticity. Jacob and Ludwig did not report the effect of unearned income on earnings, so I use an estimate from Imbens, Rubin, and Sacerdote (2001), who reported in Table 4, specification V, column 1, a marginal propensity to earn out of unearned income (MPE) of $-0.114$ with a standard error of $0.015$. In an earlier version, Imbens, Rubin, and Sacerdote (1999) reported earnings and participation elasticities of “around” $-0.20$ and $-0.14$, respectively, so I assume an intensive MPE of $\frac{d\ln \bar{w}}{dY} = -0.114 \{1 - (0.14/0.20)\} = -0.034$. On a quarterly basis, the grant should have lowered earnings by $-0.034 \cdot (6860/4) = 58.65$. Dividing the change in earnings absent the grant by the tax change gives an uncompensated elasticity of $\frac{\log(5558 - 228 + 58.65)}{\log(1) - \log(1 - 0.3)} = 0.086$. Finally, the elasticity is $\hat{\varepsilon} = \hat{\varepsilon}^{u} - \frac{d\ln \bar{w}}{dY} = 0.086 + 0.034 = 0.121$. For s.e.$(\hat{\varepsilon})$, assuming that the standard error on the intensive MPE is proportional to the error on the total MPE and that the change in income due to the grant is measured without error, then the standard error is $0.031$. For $\Delta \log(1 - \tau)$, the MTR changed from 0 to 0.30 for those receiving the housing voucher as described in footnote 29 so that $\log(1) - \log(1 - 0.3) = 0.36$.

10, 11. Gelber (2010). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are from Table 3, column 1 for men and column 2 for women. These estimates use earned income since it is less susceptible to manipulation than taxable labor income. These estimates presumably reflect primarily intensive margin responses since the extensive margin is unlikely to be important for the high-income group affected by the change in top bracket tax rates. $\Delta \log(1 - \tau)$ is the percent change in net-of-tax rate from 1989 to 1991 for the highest tax brackets reported in Table 1.

12. Saez (2010). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are from Table 2, row 1 of column 6 for wage earners with two or more children. $\Delta \log(1 - \tau)$ is the change in NTR at the first kink in the EITC benefit schedule from 1995 to 2004.

13, 14. Chetty et al. (2011b). $\hat{\varepsilon}$ and s.e.$(\hat{\varepsilon})$ are observed elasticities at middle and top kinks, calculated using equation 6 as $b/K \Delta \log(1 - \tau)$. In this equation, $K$ is the location of the tax bracket cutoff (DKr 164,300 for the middle tax and DKr 267,600 for the top tax). The estimated excess mass at the kink (b) is 1.79 (s.e. 0.05) for married women at the top kink (Figure IIIb) and 0.06 (s.e. 0.03) at the middle kink (Figure VIa). $\Delta \log(1 - \tau)$ is the size of tax changes at the middle and top tax kinks as reported in Figure II.
15. Chetty et al. (2011b). $\hat{\epsilon}$ and s.e.$(\hat{\epsilon})$ are from Table 2, column 1. \(\Delta \log(1 - \tau)\) is defined as \(2 \times \text{SD}\) of the changes in the log net-of-tax rate reported in the last row of Table 1, column 1.

**C. Top Income Elasticities**

16. Feldstein (1995). $\hat{\epsilon}$ is the high minus medium tax rate specification in Table 2. For this and other studies based on TRA86, I follow the literature in interpreting elasticities as compensated elasticities because the reform was revenue neutral. s.e.$(\hat{\epsilon})$ was not reported. For a rough estimate, rescaling the standard error cited by Feldstein on page 566 for Auten and Carroll (1994) by the ratio of sample sizes in the two studies yields s.e.$(\hat{\epsilon}) = 0.15\sqrt{14,425 / 3735} = 0.295$. \(\Delta \log(1 - \tau)\) is reported in Table 2 for the high tax rate group.

17. Auten and Carroll (1999). $\hat{\epsilon}$ and s.e.$(\hat{\epsilon})$ are from Table 2, column 6. \(\Delta \log(1 - \tau)\) was reported by Goolsbee (1999) for the highest income group in Table 3, row C for 1985–1989 because TRA86 “provided tax variation mostly at the top of the income scale, so that their overall estimates are identified primarily by reactions of high income taxpayers” (Gruber and Saez (2002, pp. 24–25)).

18. Goolsbee (1999). $\hat{\epsilon}$ and s.e.$(\hat{\epsilon})$ are from Table 4, column 1. \(\Delta \log(1 - \tau)\) is from Table 3, row C for 1985–1989 based on the quote above.

19. Saez (2004). $\hat{\epsilon}$ and s.e.$(\hat{\epsilon})$ are from Table 3C, column 3 for the top 1% of tax units. Note that Saez used gross income, not taxable income. I interpret his estimate as an intensive margin elasticity because his sample consists of repeated cross sections of workers and because the extensive margin is unlikely to be important for the top 1% of taxpayers. I interpret this estimate as a compensated elasticity following the aforementioned evidence from Gruber and Saez (2002) that income effects are small. \(\Delta \log(1 - \tau)\) is defined as \(2 \times \text{SD}\) of the log net-of-tax-rate for the top 1% of tax units listed in column 3 of Table 5.

20. Kopczuk (2010). $\hat{\epsilon}$ and s.e.$(\hat{\epsilon})$ are from Table 9, second panel, column 1, 2002–2005, with standard error imputed from the reported \(t\)-statistic. This is a compensated elasticity following Gruber and Saez (2002, equation (2)). \(\Delta \log(1 - \tau)\) is reported on page 17.

**D. Macro/Cross-Sectional**

21. Prescott (2004). $\hat{\epsilon}$ and s.e.$(\hat{\epsilon})$ were calculated by regressing log hours per worker on log net-of-tax rates using Organization for Economic Cooperation and Development (OECD) data reported by Prescott in Table 2 on hours per adult, which are converted to hours per worker using labor force participation rates from OECD Stat Extracts.\(^{34}\) The data on labor force participation

rates are missing for Canada and the United Kingdom in the 1970’s, and these observations are therefore excluded. The elasticity estimate can be interpreted as a compensated labor supply elasticity if government expenditure is viewed as unearned income in the aggregate. \( \Delta \log(1 - \tau) \) is defined as \( 2 \times SD \) of the change in log net-of-tax rate for the 12 observations with nonmissing data on hours per employed person.

22. Davis and Henrekson (2005). \( \hat{\epsilon} \) is computed using log differences in annual hours per employed adult based on the slope coefficient in Table 2.3 (middle panel, Sample C) and the sample means of annual hours per employed person and tax rates in Table 2.1 for the corresponding sample. The elasticity estimate can be interpreted as a compensated labor supply elasticity if government expenditure is viewed as unearned income in the aggregate. s.e.(\( \hat{\epsilon} \)) is calculated from the standard error reported for the slope coefficient in Table 2.3 (middle panel, Sample C). \( \Delta \log(1 - \tau) \) is computed as \( 2 \times SD \) of log 1 minus the sum of tax rates for the 19 countries in Sample C.\(^{35}\)

23. Blau and Kahn (2007). \( \hat{\epsilon} \) is computed from intensive margin (with selection correction) elasticities reported in Table 6, defining the income elasticity as the elasticity of women’s hours with respect to husband’s wages and using the Slutsky equation to compute compensated elasticities in corresponding fashion. Mean values of \( w_l \) and \( y \) are from Tables A2 and A3. I report an unweighted average of the elasticities from Model 1 for each of the three time periods. s.e.(\( \hat{\epsilon} \)) is calculated from the standard error reported for the regression coefficients in Table 7 of NBER Working Paper 11230. I assume that the covariance between the coefficient estimates is zero because the full variance–covariance matrix for the regression coefficients is not reported. \( \Delta \log(1 - \tau) \) is defined as \( 2 \times SD \) of log wage rates because the study effectively exploits cross-sectional variation in wage rates for identification; the instruments used in Table 6 correct only for measurement error. The standard deviation of log wages for married women is not reported and is, therefore, taken from Rothstein (2008), who reported a value of 0.50 in column 4 of Table 1 for married women in 1992–1993. This estimate is consistent with other published estimates of the standard deviations of women’s log wages in the Current Population Survey (e.g., Blau and Kahn (2000), Card and DiNardo (2002)).

\(^{35}\)Data are for 1995 for all countries except New Zealand and Australia, for which I use 1986 and 1985 values following Davis and Henrekson’s data appendix. Austria is excluded because Davis and Henrekson exclude it from Sample C. The variable of interest in the data set is \( tw \), which stands for “tax wedge.” See Davis and Henrekson for more details. The mean (0.496 vs. 0.500) and standard deviation (0.14 vs. 0.133) reported for Sample C in Table 2.1 differ slightly from those used in this calculation. The data were accessed from the .zip appendix at http://cep.lse.ac.uk/pubs/number.asp?number=502.
This appendix describes the sources of the values in columns 3–5 of Table II for each study. For studies 1–7, the elasticity estimates ($\hat{\eta}$) and standard errors in columns 3 and 4 are taken from Table 1 in Chetty, Guren, Manoli, and Weber (2011a); details on the sources of these estimates are given in Appendix B of that paper. Studies 8–10 are also from Chetty et al. (2011a); details on these estimates can be found in Appendix C of that paper. I follow the same methods as in Appendix B to calculate $\Delta \log(1 - \tau)$, defined here as the change in the net-of-average tax rate. The papers used for the analysis along with comprehensive documentation of the calculations are available at http://obs.rc.fas.harvard.edu/chetty/bounds_opt_meta_analysis.zip.

A. Quasi-Experimental Elasticities

1. Eissa and Liebman (1996). $\Delta \log(1 - \tau)$ is from Meyer and Rosenbaum (2000), who used the same data source and, in Table 2, calculated the financial gain from working for single mothers in 1990 as $8458, compared with $7469 in 1984. I therefore define $\Delta \log(1 - \tau) = \log(8458) - \log(7469)$.

2. Graversen (1998). $\Delta \log(1 - \tau)$ is from Table 3, which reports level changes in employment rates and participation elasticities, from which I back out $\Delta \log(1 - \tau) = (\Delta \theta / \bar{\theta}) / \hat{\eta}$, where $\Delta \theta = -0.031$ is the estimated change in employment rates for single women, $\bar{\theta} = 0.7$ is the mean employment rate for single women using an average of the six participation rates in Table 2 weighted by sample sizes, and $\hat{\eta} = -0.174$ is the elasticity estimate reported in Table 3.

3. Devereux (2004). $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD of the deviations from the mean log wage change for each region/age/education group in Table A1 for women because the variation used for identification is across region and time by education/age group. Note that this table conditions on some work, whereas in the sample used to estimate $\hat{\eta}$, nonparticipants’ wages are imputed as the average for their group.}$

4. Meyer and Rosenbaum (2001). $\Delta \log(1 - \tau)$ is from the discussion of study 4 in Chetty et al. (2011a), who defined $\Delta \log(1 - \tau) = 45\%$ after accounting for taxes and transfers as in Meyer and Rosenbaum (2000, p. 1043).

5. Eissa and Hoynes (2004). $\Delta \log(1 - \tau)$ is from Meyer and Rosenbaum (2000, p. 1043), who reported a tax change of 45% from 1984 to 1996 for the group studied by Eissa and Hoynes.

6. Liebman and Saez (2006). $\Delta \log(1 - \tau)$ is defined as $\log(1 - 0.419) - \log(1 - 0.31)$ based on the change in tax rates reported on pages 10–11 for OBRA93.

7. Blundell, Bozio, and Laroque (2011). $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD of log net-of-tax rates for participation. A standard deviation of 0.37 was obtained from personal correspondence with authors.}$
B. Macro/Cross-Sectional Elasticities

8. Nickell (2003). $\hat{\eta}$ is computed using the average point estimate of 2% (reported on page 8) and the sample means of employment rates and tax rates from Tables 1 and 2, respectively. $\text{s.e.}(\hat{\eta})$ was not reported because Nickell did not report standard errors for the studies in Table 4 on which his point estimate is based. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD}$ of log net-of-tax rates using values listed in Table 2 because most of the studies in Table 4 used in Nickell’s estimate of the effect of taxation on employment used panel or cross-sectional data for OECD countries.

9. Prescott (2004). $\hat{\eta}$ and $\text{s.e.}(\hat{\eta})$ are calculated by regressing log labor force participation rates from OECD Stat Extracts on log net-of-tax rates using the same sample of countries and years as Prescott. The data on tax rates are taken from Table 2 of Prescott. The data on labor force participation rates are missing for Canada and the United Kingdom in the 1970’s and these observations are therefore excluded. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD}$ of the change in log net-of-tax rate for the 12 observations with nonmissing data on labor force participation rates.

10. Davis and Henrekson (2005). $\hat{\eta}$ is computed using the log difference in employment based on the slope coefficient in Table 2.3 (bottom panel, Sample C) and the sample means of labor force participation and tax rates in Table 1 for the corresponding sample. $\text{s.e.}(\hat{\eta})$ is calculated from the standard error reported for the slope coefficient in Table 2.3 (bottom panel, Sample C). $\Delta \log(1 - \tau)$ is computed as $2 \times \text{SD}$ of log 1 minus the sum of tax rates for the 19 countries in Sample C.

11. Blau and Kahn (2007). For $\hat{\eta}$, I report an unweighted average of the own wage participation elasticities for each of the three time periods in Table 6, Model 1. For $\text{s.e.}(\hat{\eta})$, the standard error is calculated from the standard error reported for own log wage in Table 7 of NBER Working Paper 11230. I assume that the covariance between the coefficient estimates is zero because the full variance–covariance matrix for the parameters in the probit model is not reported. $\Delta \log(1 - \tau)$ is defined as $2 \times \text{SD}$ of log wages, calculated as described in study 23 in Appendix B above.

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*Dept. of Economics, Harvard University, Cambridge, MA 02142, U.S.A. and NBER; chetty@fas.harvard.edu.*

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