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The Effect of State Income Taxes on Home Values: Evidence from a Border Pair Study

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Abstract

State and local governments account for about 40% of all tax collections in the United States, but federal taxes command most of the attention in academic literature. This paper investigates the effect of state income taxes on home prices, and make several contributions.

First, empirically, it provides suggestive causal evidence that the elasticity of home prices with respect to state income taxes is large. Ultimately, however, the evidence is inconclusive; standard errors are large, and different specifications lead to different conclusions. This leads to a second contribution, which suggests that border-pair studies should be carefully tested before their conclusions are accepted.

Finally, using benchmark point estimates of the elasticity, the author argues that ignoring general equilibrium effects on other prices and quantities when evaluating government policy can lead to large errors in the calculation of the marginal value of public funds (MVPF) associated with a policy.

1 Introduction

State and local governments account for about 40% of all tax collections in the United States (Williams, 2012), but federal taxes command most of the attention in academic literature. In this paper, I investigate the effect of state income taxes on home prices, and make several contributions. First, empirically, I provide suggestive causal evidence that the elasticity of home prices with respect to state income taxes is large. Ultimately, however, the evidence is inconclusive; standard errors are large, and different specifications lead to different conclusions. This leads to a second contribution, which suggests that border-pair studies should be carefully tested before their conclusions are accepted. Finally, using benchmark point estimates of the elasticity, I argue that ignoring general equilibrium effects on other prices and quantities when evaluating government policy can lead to large errors in the calculation of the marginal value of public funds (MVPF) associated with a policy.

In the main empirical sections of the paper, I employ four variants of a difference-in-differences (DD) strategy: First, I consider a standard DD, regressing log home prices on log net-of-tax rates with ZIP code and time fixed effects. Second, I use the border ZIP code pair approach used by Dube, Lester and Reich (2010), in which time fixed effects are replaced by time-pair fixed effects, essentially controlling for home prices in treated ZIP code A using home prices in untreated, adjacent ZIP code B. Third, to address the issue of causation and the validity of the parallel trends assumption, I use a distributed leads/lags model, regressing log home price on 3 years' worth of leads and lags of monthly changes in the tax differential (as well as region and time-pair fixed effects). Finally, since most tax changes are small (a standard deviation of only 0.15% over the sample period), I single out two case studies that involve substantial tax changes and relatively complete home price data: Illinois's tax increase in 2011, and the District of Columbia's tax cut in 2007–2009. In these two places (separately), I regress log home price differential on the interaction of time-fixed effects and a dummy that is 1 (plus region and time-pair fixed effects). Though many variants yield large point estimates, some do not and standard errors are typically large. Thus, I find suggestive, but ultimately inconclusive, evidence of a strong relationship.

The intuition behind the conceptual insight—that the MVPF must include the general equilibrium (GE) effects of other prices and quantities—is as follows. When state income taxes rise, the state becomes a less attractive place in which to reside. To ensure housing markets clear, the price of homes within the state falls. Thus, in addition to the usual behavioral response to the increased labor tax, there is an additional general equilibrium effect on the government budget, if the government collects property taxes as well. Though this GE effect appears in both the numerator and denominator—it reduces government revenue, but through a direct transfer of those resources to individuals—it still increases the MVPF, because while the usual behavioral response to the increased labor tax is unaffected, the policy is "smaller." That is, it both raises less revenue, and reduces individual utilities by less, but revenue leakage is the same. Said another way, accounting for GE effects on home prices does not change the excess burden, in dollar terms, imposed by state income taxes but does reduce the amount of revenue collected, making the policy less attractive.

The rest of the paper is organized as follows: In the rest of this section, I discuss the relevant literature and explain more fully how this paper contributes to and advances it. Section 2 describes more fully the motivation for the paper, and the theoretical framework behind the empirical specifications. Section 3 describes the two data sources employed by the paper, and presents summary statistics. Section 4.1 presents the specification and results for the standard border-pair difference-in-differences estimator. Section 4.2 lays out the analysis of the dynamic effect of state income taxes on home prices and presents the results. Section 4.3 carefully describes the situations surrounding the two case studies and the outcomes. Section 5 discusses the implications of these results, and ties them back to the original motivation for the paper. Finally, Section 6 concludes.

Related Literature At the most superficial level, this paper might be seen as the latest in a long line of empirical papers that implement a key theoretical argument: Chetty (2009) argues that one can often use to clever theoretical arguments to show the welfare relevance of simple sufficient statistics, such as elasticities. In the article, Chetty highlights past applications of this idea in numerous areas, from tax policy to social insurance to labor economics. Hendren (2016) takes the argument one step further by noting that, in most cases, the welfare relevant statistic is simply the effect of behavioral responses to government policies on the government budget, or what Hendren calls the "fiscal externality." This observation is particularly relevant for my investigation. The obvious fiscal externality caused by higher income taxes is a reduction in taxable income that results from decreased incentives. But when the income taxes are local, the logic of compensating differentials ensures that local utility is pinned down by surrounding utility; thus, local home prices will fall, thereby decreasing property tax collections. Assuming property tax millage rates do not change in a coordinated fashion, the elasticity of home prices with respect to local income taxes is equivalent to the elasticity of property tax collections with respect to local income taxes and may be an important part of Hendren's "policy elasticity." It turns out that this effect actually shows up not as a fiscal externality, but instead as a reduction in the tax base, while holding the fiscal externality constant; thus, the fiscal externality erases a greater portion of the revenue raised by the policy.

This paper also contributes to at least three major strands of empirical literature, the first of which should be seen as an application of the above theory: a series of papers that estimate elasticities of certain real economic activity with respect to a given tax rate. A canonical example is Feldstein (1995), which estimates the elasticity of taxable income (found to be the welfare-relevant elasticity) with respect to the (marginal) net-of-tax rate. Feldstein finds a large elasticity, possibly exceeding 2, by using a small panel of taxpayers, thereby controlling for individual effects. Saez (2010) uses information contained in a single cross-section to infer this elasticity, arguing that the amount of bunching at kink points in the tax schedule maps to this elasticity. Many other similar studies, each exploiting different natural experiments, exist, including Eissa (1995), Fortin, Lemieux and Frechette (1994), and Goolsbee (2000). Saez, Slemrod and Giertz (2012) have also contributed a thorough review of this literature. This paper looks specifically at the elasticity of home prices with respect to state income tax rates.

The second investigates the capitalization of economic conditions and taxes into asset prices. Cutler (1988) applies this idea to the stock market response to the Tax Reform Act of 1986—the same Act whose effects Feldstein (1995) exploits as a natural experiment. In addition to this Act lowering top marginal personal income tax rates, it alters various aspects of corporate tax and dividend tax policy. Cutler argues that while mechanical changes in cash flows as a result of the policy changes should be correlated with excess returns, so should other aspects of the company's balance sheet. For example, repealing the investment tax credit makes new investment less attractive, thereby driving up the price of existing capital in general equilibrium; companies with a substantial stock of such capital should benefit, and the data bear this out. In the present situation, it is the home prices in the state enjoying lower income taxes that should rise in general equilibrium. Other papers demonstrating this capitalization include Friedman (2009) and Linden and Rockoff (2008), with the latter especially relevant because it focuses on home prices' response to the presence of sex offenders.

The third focuses specifically on the issue of state and local income taxes, though not through the traditional lens of welfare-relevant elasticities. One important paper in this thread is Feldstein and Wrobel (1998), which argues that states cannot redistribute income by showing that wages adjust upward (and, assuming the labor demand curve doesn't shift, the quantity of people employed adjusts downward) to compensate for higher taxes. The identification, however, comes only from instrumenting for individual tax liabilities *given* state tax rates; the state tax rates are taken as exogenous. Young and Varner (2011) directly estimate, using microdata, the tendency of the rich to emigrate to evade a high tax rate.

It finds a small propensity in the case of the New Jersey "millionaire" tax. However, the approach fails to account for the adjustment of home prices (especially home prices aimed at the wealthy) in general equilibrium, which may absorb most of the shock. The present paper takes up precisely this adjustment.

2 Theoretical Framework

2.1 Structural Model

Before starting on the empirical estimation or delving into the welfare motivation for this study, I here provide a foundation for the specification that I use in the empirical sections. This will be helpful in fixing ideas and notation before the welfare section to follow. The model, however, should be taken as primarily evocative and pedagogical rather than empirically precise.

Consider a set of individuals, indexed by $i \in \mathcal{I}$, who each have the option of living in state $j = 1$ or $j = 2$. State j has a linear tax rate τ_j^L on labor and τ_j^H on property. Individual i earns wage w_i regardless of which state he chooses to live in. Once choosing a state in which to live, he selects an amount of housing H_i to consume at pre-tax price h_j per unit; other consumption C_i has price 1. He also decides on how much work effort to supply, L_i. He faces the budget constraint

$$C_i + H_i h_j (1 + \tau_j^H) = w_i (1 - \tau_j^L) L_i.$$

Individuals differ only in the wages they earn and their idiosyncratic preference for state j, ϵ_{ij}. Conditional on choosing state j, individual i maximizes his utility

$$U_{ij} = \epsilon_{ij} \left\{ C_i^a H_i^b - \frac{\theta}{1+\gamma} L_i^{1+\gamma} \right\},$$

where $a > 0, b > 0, \gamma > 0, \theta > 0, a + b \leq 1$, subject to the budget constraint above. Solving the individual's maximization problem yields the following:

Proposition 2.1 *Individual i chooses the following policy functions:*

$$L(w(1-\tau_j^L), h_j(1+\tau_j^H)) = \tilde{L} \cdot (h_j(1+\tau_j^H))^{\frac{-b}{\gamma+1-a-b}} (w(1-\tau_j^L))^{\frac{a+b}{\gamma+1-a-b}} \tag{1}$$

$$C(w(1-\tau_j^L), h_j(1+\tau_j^H)) = \tilde{C} \cdot (h_j(1+\tau_j^H))^{\frac{-b}{\gamma+1-a-b}} (w(1-\tau_j^L))^{\frac{\gamma+1}{\gamma+1-a-b}} \tag{2}$$

$$H(w(1-\tau_j^L), h_j(1+\tau_j^H)) = \tilde{H} \cdot (h_j(1+\tau_j^H))^{\frac{-(\gamma+1-a)}{\gamma+1-a-b}} (w(1-\tau_j^L))^{\frac{\gamma+1}{\gamma+1-a-b}} \tag{3}$$

and obtains the following indirect utility:

$$V(w(1-\tau_j^L), h_j(1+\tau_j^H), \epsilon_{ij}) = \epsilon_{ij}\tilde{V} \cdot (h_j(1+\tau_j^H))^x (w(1-\tau_j^L))^y \tag{4}$$

where $x = \frac{-b(\gamma+1)}{\gamma+1-a-b}$, $y = \frac{(a+b)(\gamma+1)}{\gamma+1-a-b}$, *and* $\tilde{L}, \tilde{C}, \tilde{H}$, *and* $\tilde{V}$ *are constants, across space and individuals.*

Proof. See Appendix. ■

Thus, individual i lives in state j iff

$$\begin{aligned}
V(w_i(1-\tau_j^L), h_j(1+\tau_j^H), \epsilon_{ij}) \geq& V(w_i(1-\tau_{j'}^L), h_{j'}(1+\tau_{j'}^H), \epsilon_{ij'}) \\
\ln V(w_i(1-\tau_j^L), h_j(1+\tau_j^H), \epsilon_{ij}) \geq& \ln V(w_i(1-\tau_{j'}^L), h_{j'}(1+\tau_{j'}^H), \epsilon_{ij'}) \\
\ln \epsilon_{ij} - \ln \epsilon_{ij'} \geq& x \left[\ln(h_{j'}(1+\tau_{j'}^H)) - \ln(h_j(1+\tau_j^H))\right] + \\
& y \left[\ln(1-\tau_{j'}^L) - \ln(1-\tau_j^L)\right]
\end{aligned}$$

Suppose that the supply of housing, measured in number of housing units rather than size of house consumed,[1] in state j is S_j, and suppose $S_1 + S_2 = |\mathcal{I}| \equiv 1$ to ensure that the housing market clears exactly in aggregate. If the quantity $\tilde{\epsilon}_i \equiv \ln \epsilon_{ij} - \ln \epsilon_{ij'}$ has cumulative

[1] As discussed below in the welfare effects section, each individual will consume a different amount of housing when the net-of-tax wage and housing prices change. This can't be easily dealt with in such a simple model, so we abstract from it here.

distribution $F(\tilde{\epsilon_i})$, then the housing market clears if and only if

$$S_{j'} = F\left\{x\left[\ln(h_{j'}(1+\tau_{j'}^H)) - \ln(h_j(1+\tau_j^H))\right] + y\left[\ln(1-\tau_{j'}^L) - \ln(1-\tau_j^L)\right]\right\}$$

which means the right hand side must be constant through any reform to ensure housing market clearing. Assuming that S_j' and $F(\bullet)$ are constant also leads to the conclusion that no individual moves as a result of the policy change; the relative attractiveness of the two states remains the same for every individual, after the ensuing price change.

Allowing for $F(\bullet)$, $S_{j'}$, and the property tax rates to change over time yields a close relative of the empirical specifications I use throughout:

$$\ln\left[\frac{h_{j't}}{h_{jt}}\right] = \phi_{jj'} + \eta \ln\left[\frac{1-\tau_{j't}^L}{1-\tau_{jt}^L}\right] + \mu_{tjj'} \tag{5}$$

where

$$\begin{aligned}
\phi_{jj'} &= \mathbb{E}\left\{\frac{1}{x}F^{-1}(S_{j't},t) - \ln\left[\frac{1+\tau_{j't}^H}{1+\tau_{jt}^H}\right]\right\} \\
\eta &= -\frac{y}{x} = \frac{a+b}{b} \\
\mu_{tjj'} &= \frac{1}{x}\left\{F^{-1}(S_{j't},t) - \mathbb{E}F^{-1}(S_{j't},t)\right\} - \left\{\ln\left[\frac{1+\tau_{j't}^H}{1+\tau_{jt}^H}\right] - \mathbb{E}\ln\left[\frac{1+\tau_{j't}^H}{1+\tau_{jt}^H}\right]\right\}
\end{aligned}$$

Estimating this equation—especially η—is the purpose of the empirical sections of this paper.

2.2 Welfare Motivation

As mentioned in the Introduction, Hendren (2016) defines the marginal value of public funds (MVPF) associated with a particular policy affecting a homogeneous group of people as the ratio of their dollar-equivalent reduction in utility per dollar of revenue collected by the government for "small" versions of the policy. He argues that the MVPF for a pure tax policy can, in the absence of general equilibrium effects, be written as

$$MVPF = \frac{\text{Marginal Mechanical Revenue}}{\text{Marginal Actual Revenue}} = \frac{1}{1 - FE}$$

where FE is the "fiscal externality"—the revenue lost by the government due to behavioral response to the policy. This is derived through use of the envelope theorem. Optimal policy can be achieved by setting the social MVPF—the MVPF weighted by subjective social marginal utilities of income—equal along all possible policy paths. In the Appendix, he quickly notes that general equilibrium effects on prices should be included if they exist. Here, I take up the subject of what that looks like in this particular case.

Consider increasing the labor tax rate slightly in a particular state, by ϵ. Recalling from the previous subsection that, in the short run, no one moves as a result of this policy, this has two effects on an individual's utility, after applying the envelope theorem: its mechanical tax effect, and its general equilibrium effect on housing prices in his chosen state.[2] The dollar-equivalent utility reduction from the first effect is simply $\epsilon w_i L(w_i(1-\tau_j^L), h_j(1+\tau_j^H))$. The dollar-equivalent utility increase from the second effect is $\frac{\epsilon}{1-\tau_j^L}\eta h_{jt}(1+\tau_j^H)H(w_i(1-\tau_j^L), h_j(1+\tau_j^H))$. However, this also has an effect on whoever owns the property, and leases it to the individual; it decreases his revenue by $\frac{\epsilon}{1-\tau_j^L}\eta h_{jt}H(w_i(1-\tau_j^L), h_j(1+\tau_j^H))$.[3]

Moving on to the government budget, the increase in tax has three separate effects: the mechanical revenue effect, the mechanical effect of the drop in home prices, and the behavioral response to these price changes. The behavioral response can be further decomposed into four effects: the labor supply and housing consumption effects of the changes in net-of-tax wage and home prices. The mechanical effects are $\epsilon w_i L(w_i(1-\tau_j^L), h_j(1+\tau_j^H))$ and $-\frac{\epsilon}{1-\tau_j^L}\eta h_{jt}\tau_j^H H(w_i(1-\tau_j^L), h_j(1+\tau_j^H))$, respectively. The behavioral responses' effects on

[2] In this exercise, I assume that any change in the *difference* in log housing prices occurs in the state adjusting its policy. Presumably there are "third party" states as well, which pin down log housing prices in the second state.

[3] I assume the landlord has no behavioral response, and is merely a large, risk-neutral corporation that rebates his profits lump-sum to individuals.

the budget are as follows:

$$\begin{aligned}
\text{Labor Supply to Wage} \quad & -\frac{\epsilon}{1-\tau_j^L} e_L^w w_i \tau_j^L L(w_i(1-\tau_j^L), h_j(1+\tau_j^H)) \\
\text{Labor Supply to Housing Prices} \quad & -\frac{\epsilon}{1-\tau_j^L} \eta e_L^h w_i \tau_j^L L(w_i(1-\tau_j^L), h_j(1+\tau_j^H)) \\
\text{Housing Consumption to Wage} \quad & -\frac{\epsilon}{1-\tau_j^L} e_H^w h_j \tau_j^H H(w_i(1-\tau_j^L), h_j(1+\tau_j^H)) \\
\text{Housing Consumption to Housing Prices} \quad & -\frac{\epsilon}{1-\tau_j^L} \eta e_H^h h_j \tau_j^H H(w_i(1-\tau_j^L), h_j(1+\tau_j^H))
\end{aligned}$$

where e_L^w is the (direct) elasticity of labor supply with respect to net-of-tax wage, and other es are defined similarly.

Notice that we can replace $w_i \tau_j^L L_i$ with R_i^L, the revenue from the labor tax collected from individual i and $h_j \tau_j^H H_i$ with R_i^H, the revenue from the property tax collected from individual i. Thus, we have arrived at the following proposition:

Proposition 2.2 *The MVPF of raising the state income tax is given by*

$$MVPF = \frac{\frac{1}{\tau^L} R^L - \frac{\eta}{1-\tau^L} R^H}{\frac{1}{\tau^L} R^L - \frac{1}{1-\tau^L} \left[\eta R^H + \left(\underbrace{e_L^w + \eta e_L^H}_{\text{Total response}} \right) R^L + \left(\underbrace{e_H^w + \eta e_H^h}_{\text{Total response}} \right) R^H \right]} \tag{6}$$

If housing is not considered, except that the total labor supply behavioral response $(e_L^w + \eta e_L^h)$ is correctly measured, then the MVPF would simply reduce to

$$MVPF = \frac{1}{1 - \frac{\tau^L}{1-\tau^L}(e_L^w + \eta e_L^h)}$$

which is the standard form.

After empirically estimating η in the ensuing sections, I will return to this MVPF calculation in Section 5.

3 Data

This paper primarily employs data from two sources: Home price data comes from Zillow Research (www.zillow.com/research/data/), while tax data comes from published output of the NBER TAXSIM model (users.nber.org/∼taxsim/). These data are supplemented with population and geography data from the Census for counties and from ESRI for ZIP codes.

3.1 Population and Geography

Data on the population (in 2014), location, and area of all U.S. ZIP codes was taken from ESRI data that ships with ArcGIS; the data identify 30,450 ZIP codes with geographic meaning in the U.S. The adjacency matrix was then computed using ArcMap 10.2.[4] I define a *border pair* as a pair of ZIP codes that border each other but are not members of the same state. A handful of ZIP codes do cross state lines, but the ESRI data allocates each ZIP code to a single state.

3.2 Home Prices

Zillow Research publicly publishes various home price indices at the state, metro, county, city, ZIP code, and neighborhood levels. These home price indices include various percentiles of home prices, condo prices, single-family home prices, value per square foot, and prices for various subsets of single family homes. In this paper, I consider only the median home prices and the median value per square foot, the latter of which is more closely tied to h_{jt} in Equation 5. These data are monthly from April 1996 through the present, though not every ZIP code has data in any given month.

Figure 1 shows the availability of median home price data by ZIP code. While most

[4] It is worth noting that ZIP codes are not actual places or geographical polygons but, rather, lists of addresses. In general, these addresses are geographically clustered, but some of them are a single point (a P.O. box, for instance) and some are not contiguous. The Census, instead of the actual ZIP codes, uses ZCTAs—ZIP code tabulation areas—which are geographical polygons. ESRI doesn't disclose exactly how these polygons are computed, but cites TomTom in the credits.

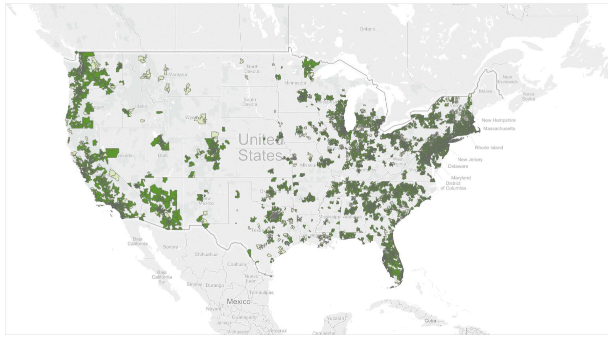


Figure 1: Availability of Zillow data. Dark areas have median home price data since April 1996, while light areas do not have median home price data then but do by August 2015. The lightest areas have no home price data.

of the geographical area of the country is not covered by the dataset, there is substantial coverage of the most populated areas of the country. Specifically, Figure 2 shows that the vast majority of the country's population is covered by these data as far back as 1997, with increasing coverage since then. Over 50% of that population living in a ZIP code on a state border is covered for the entire sample period, and over 60% by 2006.

Summary statistics for the Zillow data can be found in Table 1. As in Dube, Lester and

Table 1: Home price summary statistics. Statistics for the entire sample of available counties/ZIP codes, as well as for the subsample of counties/ZIP codes that are members of a border pair, are reported.

	1997			2015		
	count	mean	sd	count	mean	sd
All ZIP Codes						
Median Value	11320	121380	72849	12938	240500	228085
Median Value per Sq. Ft.	11342	75	34	12752	149	130
ZIP Codes in Border Pair						
Median Value	554	123836	71090	784	231692	216681
Median Value per Sq. Ft.	557	72	26	717	138	105

Reich (2010), I present summary statistics from the home price data using two samples: the entire sample of ZIP codes for which data is available, and the sample that includes only those ZIP codes that are part of a border pair. In both cases, one can see that I have similar coverage in the "median value" and "median value per square foot" variables. The samples

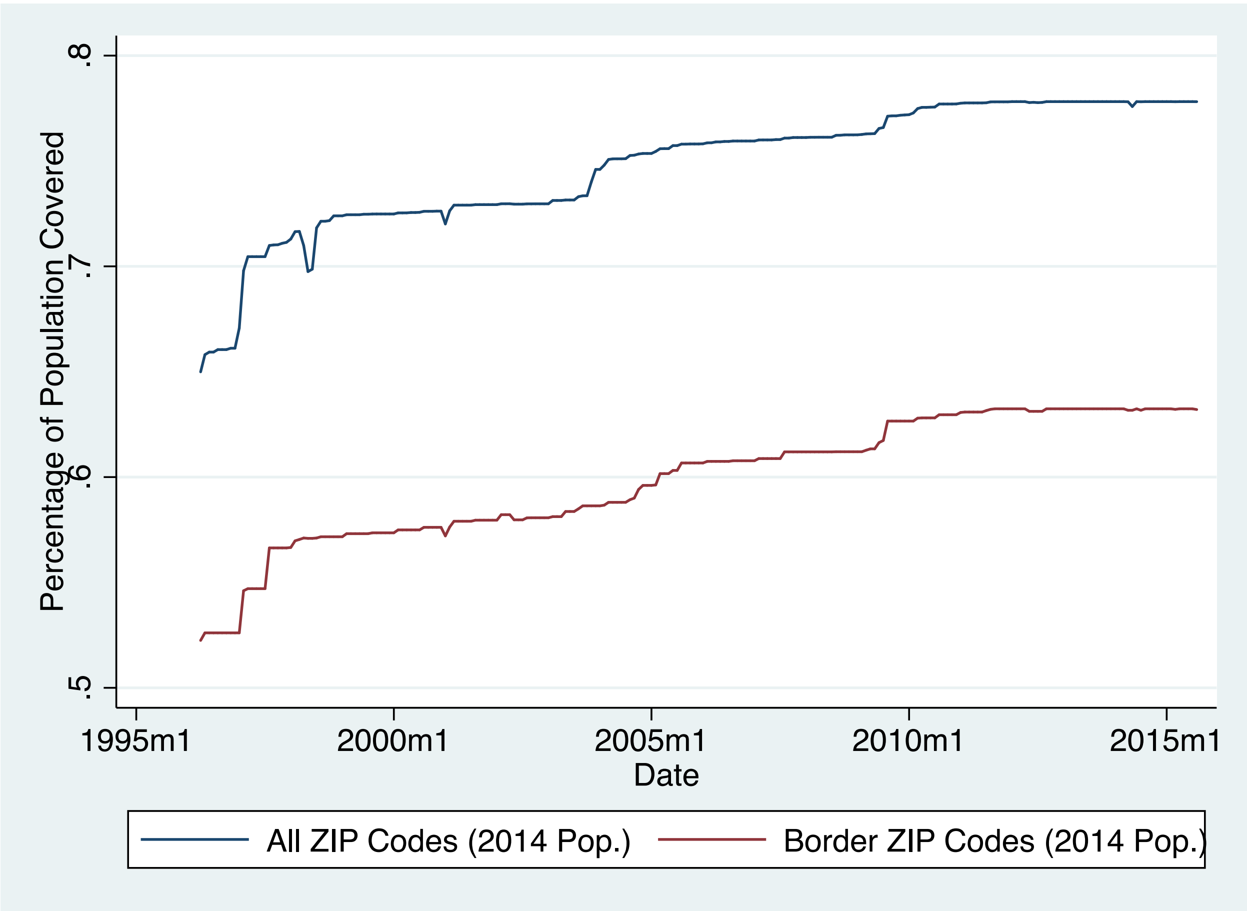


Figure 2: Population covered by Zillow data. In each month, the 2014 populations of the ZIP codes which Zillow covers in that month are summed and divided by the sum of the 2014 populations of *all* ZIP codes to arrive at the point on the graph. Thus, the upward trend in the graph is due only to the increasing quality of data, and not to population growth.

are large, with even the smallest sample (1997 data on ZIP codes in a border pair) containing 554 ZIP codes. Additionally, the data appears to be fairly representative, with the values at least qualitatively in line with national home prices.

3.3 Taxes

NBER's TAXSIM model (Feenberg and Coutts, 1993) is a piece of software that calculates tax liabilities for tax units, given the various data that would be collected on a tax return. In addition to being able to calculate such liability for any hypothetical tax unit a researcher might wish to study, various liabilities of interest have already been calculated and published on the Web for every state and year from 1977 through 2010.

The theoretically relevant tax liability for a household deciding its state of residence is the *total*, not marginal, tax that would be owed in the states up for consideration. For robustness, I consider three measures of this, all of which are provided in the published TAXSIM tables. Before I detail these three measures, however, it is worth noting that whether I use the state tax rate or the total (state plus federal) tax rate is irrelevant. Since state taxes are deductible on federal tax returns but not vice-versa,[5] the net-of-tax total rate is $1 - \tau_T = (1 - \tau_f) \cdot (1 - \tau_s)$. Taking logs (which I do in all of my specifications, so that regression coefficients can be interpreted as elasticities), we have $\log(1 - \tau_T) = \log(1 - \tau_f) + \log(1 - \tau_s)$. Since all specifications involve a time fixed effect, $\log(1 - \tau_f)$ can be dropped.

For the first two measures, I consider the state tax owed by a typical family with two adults and two children at nominal incomes of \$50,000 and \$75,000. The dataset makes assumptions about the nature of such families' income (what percentage is wages, the extent of their deductions, etc.). Since I've chosen to use the taxes owed at a constant nominal income, changes in the taxes owed are due only to changes in tax law and not to inflation.[6] I've chosen a 4-person family because families comprise the primary market for owner-occupied housing.

[5] In a few states, federal tax is deductible on the state tax return, but this is accounted for in the data.

[6] A few states, as of 2014, do index their brackets to inflation, but they are in the minority. Additionally, inflation has been low throughout the sample period, so the indexing of the brackets in these states will be relatively unimportant.

These incomes I've chosen represent the 50th and 68th percentiles, respectively, of the 2010 U.S. income distribution, and therefore are probably typical of prospective home-buyers.

While median home prices reflect the preferences of a typical home-buyer, rather than an income-weighted home-buyer, that is only true for the market for a given county or ZIP code. Some counties or ZIP codes are marketed to significantly wealthier households. Thus, I've also used a dollar-weighted measure as my third measure to account for affects on wealthier home-buyers and their homes. The measure I use considers a fixed sample of taxpayers over time, but adjusts the earnings by inflation plus 1.4% annual real growth. It then reports the dollar-weighted average tax rate for every state.

Table 2 presents summary statistics on the average tax rates, as well as their year-over-year changes. The levels clearly have lots of heterogeneity (standard deviations around 1.5%), while the year-over-year changes are more homogeneous (standard deviations around 0.15%). It is this homogeneity that will lead to some of the methodological issues this paper presents. Note, however, that the standard deviation of the year-over-year changes is still large relative to the mean.

Table 2: Tax summary statistics. Statistics on levels in 1997 and 2010 are presented, followed by statistics on the pooled year-over-year changes for all years since 1997. All numbers are average state rates for the populations indicated.

	1997			2010		
	count	mean	sd	count	mean	sd
Levels						
Family of 4 with $50,000 Income	51	2.983	1.808	51	2.360	1.459
Family of 4 with $75,000 Income	51	3.521	1.976	51	3.040	1.689
Dollar-Weighted	51	3.106	1.656	51	3.097	1.609
Year-Over-Year Changes, All Years Since 1997						
Family of 4 with $50,000 Income	663	-0.0479	0.160			
Family of 4 with $75,000 Income	663	-0.0370	0.136			
Dollar-Weighted	714	0.00303	0.145			

4 Empirical Results

4.1 Static Results

The empirical goal of this paper is to estimate Equation 5, which I reproduce here for convenience:

$$\ln\left[\frac{h_{j't}}{h_{jt}}\right] = \phi_{jj'} + \eta \ln\left[\frac{1-\tau^L_{j't}}{1-\tau^L_{jt}}\right] + \mu_{tjj'}$$

where j and j' are two regions, h_{jt} is the home price in region j at time t, τ^L_{jt} is the labor tax rate in region j at time t ,and

$$\mu_{tjj'} = \frac{1}{x}\left\{F^{-1}(S_{j't},t) - \mathbb{E}F^{-1}(S_{j't},t)\right\} - \left\{\ln\left[\frac{1+\tau^H_{j't}}{1+\tau^H_{jt}}\right] - \mathbb{E}\ln\left[\frac{1+\tau^H_{j't}}{1+\tau^H_{jt}}\right]\right\}$$

is the error term. Undoing the spatial difference[7] inherent in this equation begets

$$\ln h_{jt} = \phi_j + \eta \ln(1-\tau^L_{jt}) + \mu_{jt}.$$

Replacing $\mu_{jt} = \psi_t + \mu_{jt}$, where $\mathbb{E}[\mu_{jt}|t] = 0$, yields a standard difference-in-differences equation:

$$\ln h_{jt} = \phi_j + \psi_t + \eta \ln(1-\tau^L_{jt}) + \mu_{jt}. \tag{7}$$

Estimates of this specification can be found in Table 3. Clearly, the relationship is economically significant, though the standard errors are large enough to make meaningful inference impossible. We will see that this is a common problem throughout. More important, these estimates are valid only if μ_{jt} is uncorrelated with the local tax rate, conditional on the region and time fixed effects. $\mu_{tjj'}$ encapsulates three economic forces: pure relative preference between the regions, the relative supply of housing in the two regions, and the difference in property taxes. Property tax rates are highly local, whereas income tax rates vary mostly

[7]For proper estimation of standard errors, discussed below, I will need to cluster on both the border segment (the pair of states) and the state of a given observation. This is only possible if I separate the two sides of this difference.

Table 3: Estimates of Equation 7 using the full ZIP code sample. All dependent and independent variables are in terms of logs. The unit of observation is ZIP code-month. Coefficients should be interpreted as elasticities. All independent variables are average state net-of-tax rates on the population listed. All models include ZIP code and time fixed effects. Standard errors, clustered at the state level following Bertrand, Duflo and Mullainathan (2004), are in parentheses.

	Median Home Price			Per Sq. Ft. Home Price		
Family Earning $50k	9.535			10.316		
	(5.403)			(5.258)		
Family Earning $75k		14.054			14.485	
		(6.331)			(6.166)	
Dollar-Weighted			4.558			4.950
			(4.542)			(4.733)
Observations	2,135,277	2,135,277	2,290,355	2,132,499		2,285,473

at the state level. Thus, the two tax rates are unlikely to covary. Supply may well respond positively to a positive shock to a region's net-of-tax rate, but any such response should dampen the housing price response, and my estimates that do not account for this should therefore be biased toward zero.

Most concerning is possible correlation between relative net-of-tax rates and pure preference between the two regions, which is likely to be present. Regions that become more attractive for reasons having nothing to do with taxes might simultaneously see a drop in tax rates, as the government can pay its expenses with a lower rate during a local boom. Thus, estimates of Equation 7 incorrectly interpret this correlation as a causal effect of tax rates on home prices.

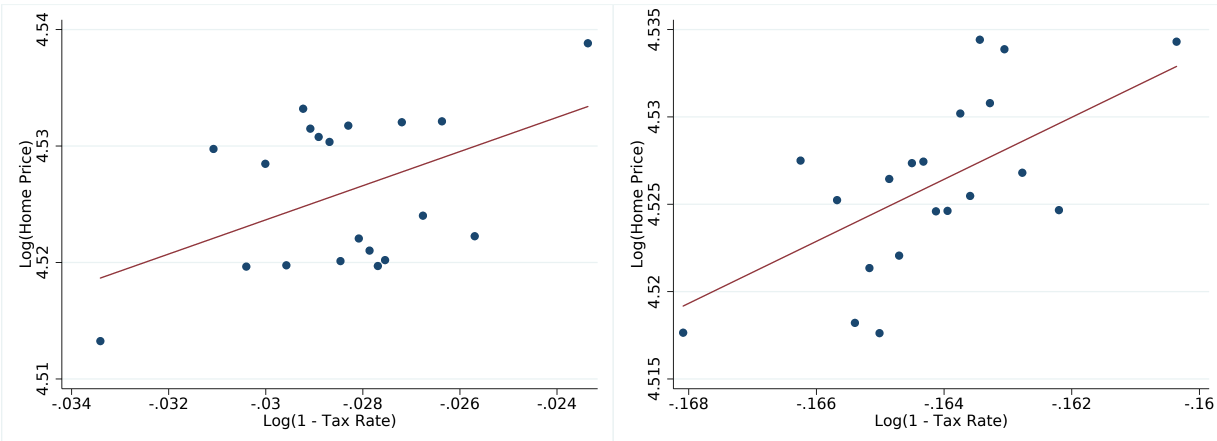
To combat this, I propose border pair, difference-in-differences techniques. ZIP codes that are neighboring, but in different states, should experience similar shocks to their pure desirabilities, meaning relative preferences should not change much. This can be accounted for with a pair-time fixed effect, which controls for general conditions in the area, including desirability. I employ the classic border pair specification proposed in Dube, Lester and

Reich (2010) is

$$\ln h_{jpt} = \phi_j + \psi_{pt} + \eta \log(1 - \tau_{jt}) + \mu_{jpt}, \tag{8}$$

where p indexes the border pair and ψ_{pt} is a pair-time fixed effect.

Figure 3 shows binscatters of log per-square-foot price versus net-of-tax rates, controlling for ZIP code and pair-time fixed effects. The relationship appears to be strong. Table 4



(a) Family Earning \$50,000

(b) Dollar-Weighted

Figure 3: Per-square-foot home prices vs. net-of-tax rates for two different definitions of tax rate. Both variables have had logs taken, meaning the slope should be interpreted as an elasticity. The specification includes ZIP code and pair-time fixed effects.

presents the regression results. The residuals are subject to various types of correlation across observations as noted by Dube, Lester and Reich (2010). First, home prices are measured monthly and at the ZIP code level, while taxes are measured annually and at the state level. This implies errors may be correlated for two observations within the same state-year. In addition, pairs along a border segment (pair of neighboring states) may mechanically have correlated errors, since the same ZIP code will appear multiple times—one for each border pair of which it is a part. Since each pair also is subject to serial autocorrelation, this means that observations on the same border segment may be correlated *even at different points in time*. Thus, I follow Dube, Lester and Reich (2010) and Conley and Taber (2011) and cluster separately at the border segment and state levels.

Point estimates of the elasticity of home prices with respect to the net-of-tax rate range

Table 4: Estimates of Equation 8. All dependent and independent variables are in terms of logs. The unit of observation is ZIP code-pair-month, meaning each ZIP code-month will appear multiple times, depending on the number of pairs of which it is a part. Coefficients should be interpreted as elasticities. All independent variables are average state net-of-tax rates on the population listed. All models include ZIP code and pair-time fixed effects. Standard errors, clustered separately at the border segment (pair of states) and state levels, are in parentheses.

	Median Home Price			Per Sq. Ft. Home Price		
Family Earning \$50k	1.566			1.466		
	(0.933)			(0.930)		
Family Earning \$75k		1.814			1.612	
		(0.932)			(0.892)	
Dollar-Weighted			2.130			1.775
			(1.213)			(1.257)
Observations	205,338	205,338	222,064	198,750	198,750	214,206

from 1.466 to 2.13 across these specifications, all of which are large. However, the large standard errors make meaningful inference difficult. I will discuss these standard errors further in Section 5, but they mostly result from two phenomena: clustering at the segment and state levels throws out meaningful time series information about the dynamic response to taxes; and most of the tax changes are quite small, which is equivalent to few treated clusters. I address the former issue in Section 4.2 as a side effect of testing the parallel trends assumption, and the latter in Section 4.3.

4.2 Dynamic Response

The identification assumption necessary for the validity of the strategy in Section 4.1 is commonly known as parallel trends. That is, suppose ZIP code i is a member of one state that changes its tax rate from year t to year $t+1$, and ZIP code j is a neighboring member of another state that does not. Equation 8 considers ZIP code j to be a control group for ZIP code i. The ZIP code fixed effects control for the possibility that ZIP code j is secularly (irrespective of taxes) more or less desirable than ZIP code i, but only if the gap between them would be expected[8] to have been constant in the absence of the tax change. In the language of Section 2.1, changes in tastes captured by $F(\bullet)$ must not correlate with changes in relative tax rates.

The parallel trends assumption can be tested by looking for the dynamic response to the tax change. Specifically, one would expect

$$\frac{\partial \ln h_{j,t+s}}{\partial \ln(1-\tau_{jt})} \neq 0$$

iff $s \geq 0$. That is, the effect of a tax change should only be felt on or after the date of its passage.[9] Additionally, investigating the dynamic response to the tax change allows extraction of more information from the sample. Specifically, the CRVEs of the previous section merely use a single measure of the correlation of taxes and home prices within a segment, controlling for fixed effects. But the data within each cluster contains more information than that—it contains the *timing* of home price changes, and that information is useful for forming a conclusion about the home price response to taxes if it is systematically related to the timing of tax changes.

Dube, Lester and Reich (2010) suggest the following specification for estimating the dynamic effect:

$$\ln h_{ipt} = \phi_i + \psi_{pt} + \sum_{s=-(T-1)}^{T} (\eta_{-s} \Delta \ln(1-\tau)_{i,t+s}) + \eta_T \ln(1-\tau)_{i,t-T} + \mu_{ipt}. \tag{9}$$

As they note, specifying the tax variables in first-differences over time allows estimation of elasticities η_s of home prices in period $t+s$ with respect to a permanent tax change in period t.

Even though taxes change only yearly, I have monthly home price data, so I can estimate

[8] It is important that parallel trends need only hold in expectation. Idiosyncratic violations of parallel trends won't affect the point estimate, and will just increase the standard error. To bias the result, the violations of parallel trends must correlate with the independent variable—the difference in tax rates.

[9] or anticipated passage. I will return to this idea later.

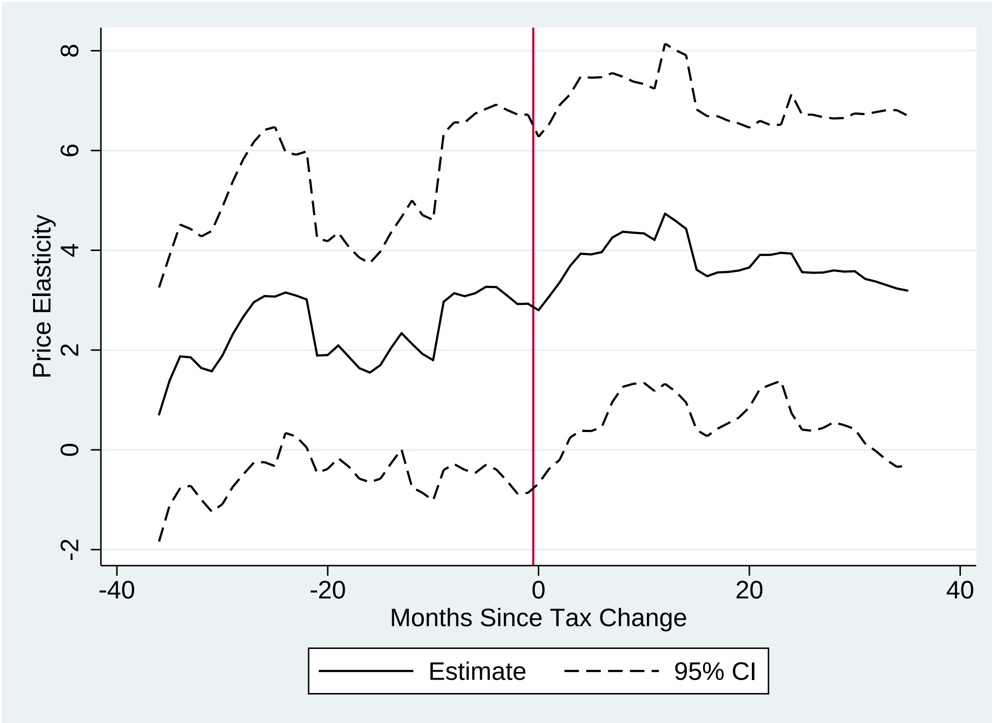
a monthly home price response. Choosing T presents a tradeoff; the larger T is, the better the picture of the dynamic response, but the smaller the sample: For an observation in period t to be included in the sample, the original dataset must contain T periods on both sides of t. Given that my tax data runs through 2011 (for dollar-weighted taxes) but home prices run through 2014 and into 2015, I've chosen 3 years ($T = 36$) as a compromise position.

I estimate Equation 9 six ways: median and per square foot prices as the dependent variable; and the three tax measures as the independent variable. I continue to use standard errors clustered separately at the state and border segment levels. Figure 4 visualizes these estimates for two of these specifications. Others yield qualitatively similar results and can be found in Appendix B. In general, the results strongly reject a null hypothesis in the case of dollar-weighted taxes, as home prices respond strongly to tax changes after adoption but not before. For other measures of taxes, however, the estimates are borderline significant at many leads and lags, suggesting either a violation of parallel trends or a lack of evidence of any relationship, depending on the interpretation. I will discuss possible explanations and implications of this in Section 6.

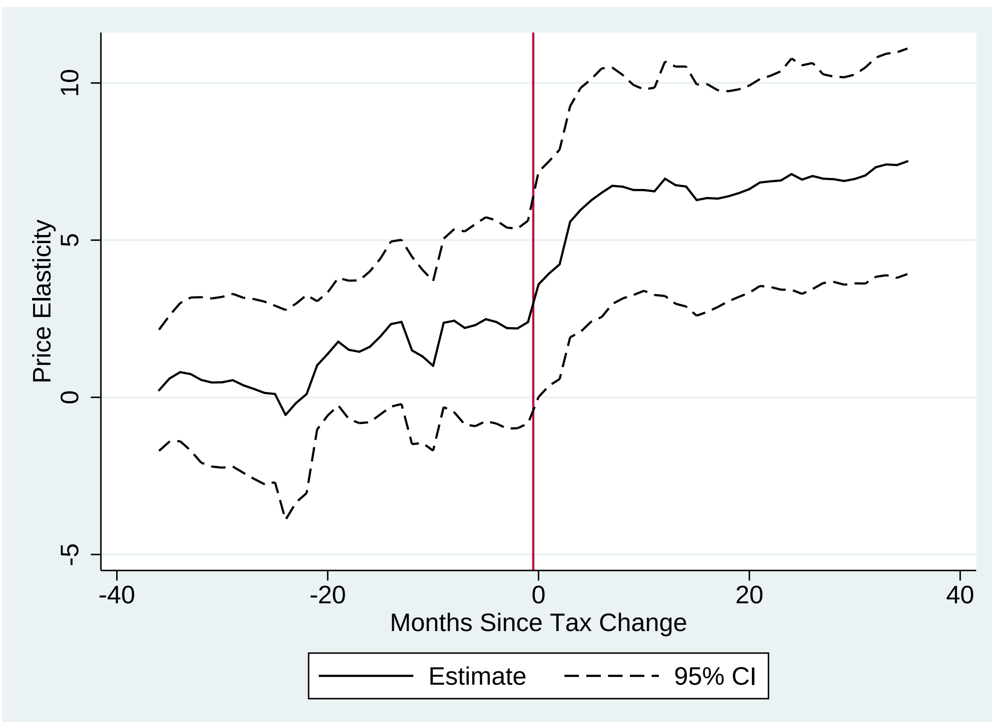
4.3 Event Study

One shortcoming of the data discussed in Section 3.3 is that most year-over-year tax changes in the sample period were quite small, with a standard deviation of only roughly 0.15%. This presents a major problem of external validity, in addition to making CRVEs unreliable (Conley and Taber, 2011). Even a sizable elasticity like 5, estimated in Section 4.2, corresponds to an increase in home values of under 1% for a tax cut of 0.15%. Extrapolating this to a 5% increase in home values for a tax cut of 1% seems unjustified. One way to examine the validity of such an extrapolation is to use an event study approach, examining those instances in which states changed their tax rates markedly relative to their neighbors. I focus on only two such instances in this analysis:

- Illinois raised taxes by 1.8% in dollar-weighted terms in 2011.



(a) Taxes on families earning \$50,000, per square foot home prices



(b) Dollar-weighted taxes, per square foot home prices

Figure 4: Dynamic price responses to tax changes via estimation of Equation 9. Elasticities of home prices in month $t + s$ with respect to a permanent change in the net-of-tax rate in month t are given by the solid line, with 95% confidence intervals bounded by the dashed lines. Standard errors are clustered separately at the border segment and state levels.

- The District of Columbia reduced taxes in 2007 (phased in through 2009) by 1% in dollar-weighted terms, and by almost 2% for families earning $50,000.

The criteria for inclusion were: a tax change of at least 1% in dollar-weighted terms over a 3 year period,[10] and whether I have a substantial number of border county or border ZIP code pairs for the state in question. Besides the above two instances, one other instance met these criteria according to the TAXSIM data: Michigan in 2010 had seen a rise in dollar-weighted taxes of 1.11%. However, in looking at the taxes on various income groups, the maximum state rate on wages, and various news stories from the time, I could find no evidence of such a large tax hike. Thus, I have excluded this instance from my analysis.

Illinois 2011 In 2011, Illinois raised income taxes by 2% across the board. The bill was passed on January 12, 2011, retroactive to January 1, 2011 (Henchman and Padgitt, 2011). According to Long (2011), the bill was passed with the state's budget under duress. It passed with an incredibly close vote and therefore was unlikely to be fully anticipated. Illinois has no local income taxes that might confound this tax change. On the other hand, the bill provides for a phase-out, and so might be regarded as temporary by residents. Data on the Illinois borders is patchy, but there are many data points.

Washington, D.C., 2007 According to both the TAXSIM ;[[]]data and the Tax Foundation, Washington, D.C., income taxes fell substantially from 2006 to 2009—0.5% (9% to 8.5%) for high earners, 1.5% (7.5% to 6%) for moderate earners, and 1% (5% to 4%) for the poor. Unlike the Illinois case, I can find no press coverage of this change, which leaves me with little background on how well it might have been predicted, or under what circumstances. In fact, newspaper articles covering the current round of tax cuts in Washington, D.C., remark that it is the "first big tax cut in 15 years" (Davis, 2014), suggesting that the tax cuts of 2007–2009 might have been passed far in advance or automatically triggered. Simultaneous with these changes has been a series of tax increases in Maryland, focused

[10] Section 4.2 suggests dollar-weighted taxes cause the most response.

especially on the wealthy (The Washington Post, 2014); however, the cause of any home prices should be seen as the change in the *relative* taxation of Maryland and Washington, D.C., meaning that these tax increases might decrease the elasticity for a given price change, but shouldn't change the interpretation. Data for this area is almost perfect over the sample period.

To avoid issues resulting from unbalanced panels while viewing the dynamic response to the policy change, I conduct the event study by using a regression on ZIP code pairs:

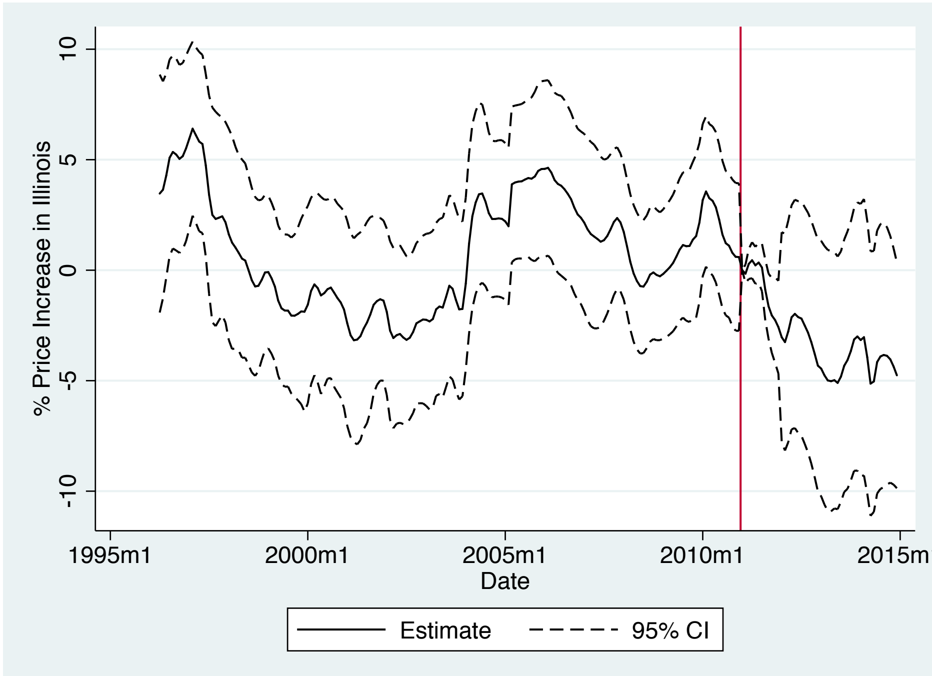
$$\ln h_{ipt} = \phi_i + \psi_{pt} + \sum_{s=T_1}^{T_2} \eta_s \mathbb{1}[t = s] \times TREAT_i + \mu_{ipt}, \tag{10}$$

where $TREAT$ represents whether ZIP code i belongs to the treated state (Illinois or D.C.). I omit the interaction term for the month during which the tax change took place, so all ηs represent the *change* in home prices relative to the month during which the event took place.

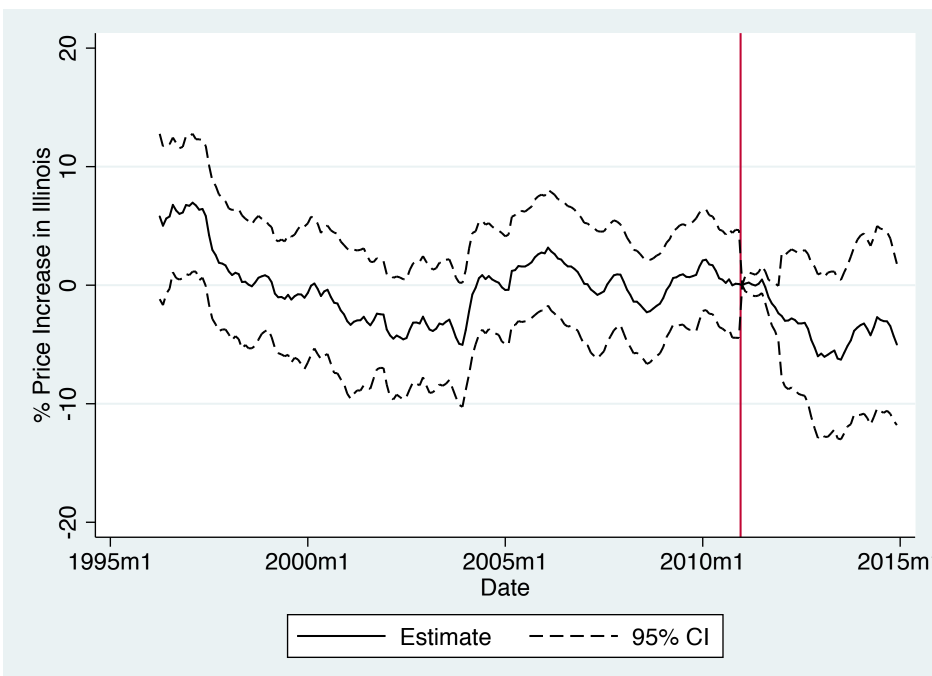
Standard errors are challenging. Continuing to cluster errors at the segment and state level separately results in only a handful of clusters, which is not sufficient to estimate standard errors. I do cluster at the ZIP code-year level to correct for mechanical correlation between the error terms due to repetition of a ZIP code as a member of multiple pairs, and since taxes change only yearly.[11]

Figure 5 depicts results for Illinois, and Figure 6 depicts results for Washington, D.C. Turning first to Illinois, median home prices appear to have been declining (relative to neighbors) for several years prior to 2011, invalidating the parallel trends assumption. The point estimate for per square foot home prices matches expectations better; it is fairly flat prior to the tax change and then drops by about 5%. However, in all cases, 95% confidence intervals rarely exclude zero in either pre- or post-periods, meaning not much can be extracted from this example.

[11] Clustering at the ZIP code level, which corrects for any serial correlation of the error term, results in too few clusters to estimate the standard errors in a model with many regressors.

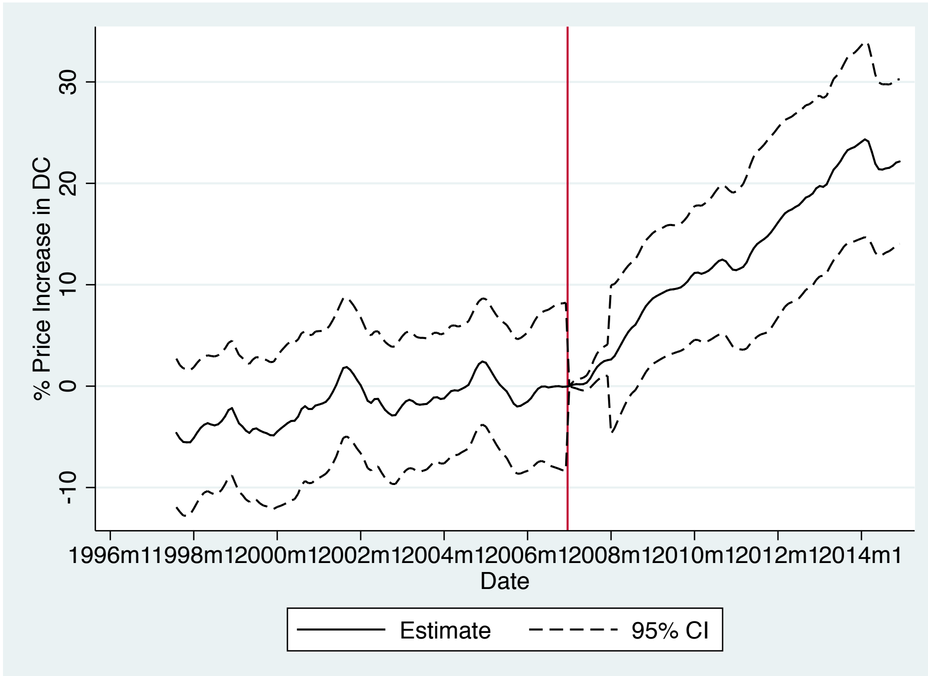


(a) Median home prices

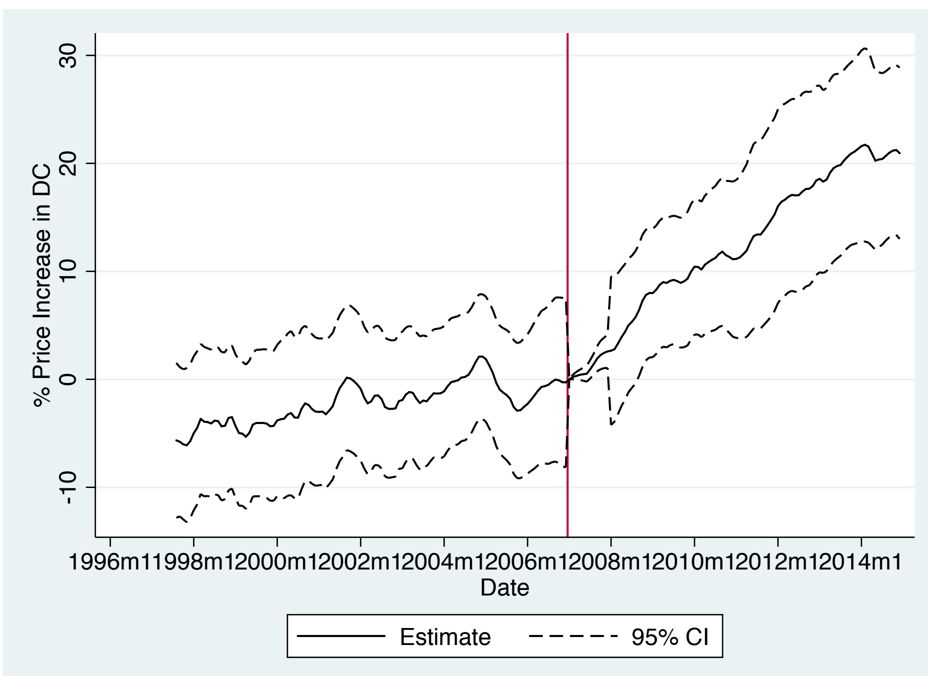


(b) Per square foot home prices

Figure 5: Illinois event study. The estimate of η_s from Equation 10 is given by the solid line, with 95% confidence intervals bounded by the dashed lines. These values are normalized such that the value is 0 in January 2011, the month of the tax change, marked by a vertical line. Standard errors are clustered at the ZIP code-year level.



(a) Median home prices



(b) Per square foot home prices

Figure 6: D.C. event study. The estimate of η_s from Equation 10 is given by the solid line, with 95% confidence intervals bounded by the dashed lines. These values are normalized such that the value is 0 in January 2007, the month of the tax change, marked by a vertical line. Standard errors are clustered at the ZIP code-year level.

However, the results for Washington, D.C., are a textbook example of an event study. Almost exactly contemporaneously with the beginning of the tax cut in 2007, home prices in Washington, D.C., began to rise rapidly relative to neighboring ZIP codes. These results are large in magnitude—about 10% over 3 years, relative to a 1% dollar-weighted tax cut[12]—and highly significant.

5 Discussion

5.1 Alternative Standard Errors

As mentioned, I follow Dube, Lester and Reich (2010) and use standard errors that are clustered at the border segments level and state level separately. Each dimension has over 40 clusters, making inference generally reliable. Furthermore, Bertrand, Duflo and Mullainathan (2004) show that clustering at the state level (their setting features no border pairs) is required to account for quite general patterns of serial correlation. These standard errors are, unfortunately, large enough to prohibit detection of reasonable-sized effects in many specifications.

It might appear that one could refine the sample to obtain more efficient estimates. In particular, the identification assumption required for validity of the border pair approach is that, conditional on pair-time and ZIP code fixed effects, home prices would have evolved in the same way on both sides of the border in the absence of any tax change. This is conceptually equivalent to the assumption of matched pairs, and it is standard in such studies to cluster at the pair level, which still allows for arbitrary serial correlation within the pair.

The complexity of the geographic map, however, means that the same ZIP code will appear in more than one pair. This clearly leads to mechanical correlation across all pairs that share a common ZIP code. But it is worse than that; due to the pair-time fixed effects,

[12] although the reader should keep in mind the simultaneous tax increases in Maryland

Table 5: Estimates of Equation 8 using a sample of border pairs such that no ZIP code appears in more than one pair. All dependent and independent variables are in terms of logs. The unit of observation is ZIP code-month. Coefficients should be interpreted as elasticities. All independent variables are average state net-of-tax rates on the population listed. All models include ZIP code and pair-time fixed effects. Standard errors, clustered at the pair level, are in parentheses.

	Median Home Price			Per Sq. Ft. Home Price		
Family Earning \$50k	1.918			1.737		
	(0.629)			(0.645)		
Family Earning \$75k		2.015			1.744	
		(0.700)			(0.693)	
Dollar-Weighted			2.967			2.292
			(0.806)			(0.819)
Observations	2,135,277	2,135,277	2,290,355	2,132,499		2,285,473

this can lead to a chained effect that causes mechanical correlation of *all* observations along a given border or within a given state. This leads to the two-way CRVE implemented in the previous section. This complexity can be removed, however, by ensuring that each ZIP code appears as part of only one border pair. Once this is guaranteed, there should be no mechanical correlation of the error term outside a border pair.

Table 5 shows the results of reestimating Equation 8 using this method. In particular, I have randomly sorted the list of all adjacent pairs or ZIP codes, and then iteratively eliminated pairs until each ZIP code appears in only one pair. I cluster the standard errors at the pair level, as is typical of matched pairs tests. The point estimates are qualitatively similar, suggesting that the slimming of the sample did not meaningfully affect the estimates, but with much smaller standard errors; as a result, the estimates are highly significant.

However, it turns out that this method substantially over-rejects a null hypothesis. I test this by following Bertrand, Duflo and Mullainathan (2004) and generating 1,000 placebo policies. States indpendently have a 50% chance of receiving a placebo treatment, and then the timing of that treatment is randomly and independently assigned. A valid statistical test of size .05 should reject the null hypothesis of zero effect of the placebo treatment about 5% of the time. This procedure, however, rejects the null over 20% of the time. Meanwhile, the two-way clustered standard errors of the previous section lead to rejection of the null for a placebo treatment about 7% of the time—much closer to the proposed size of the test.

The difference in these two estimates of the standard error is allowance for the possibility of correlation of the error term across different ZIP codes on the same side of a state border, which might be attributable to statewide shocks that do not propogate beyond the state border, such as state laws. So long as these shocks are not systematically correlated with state income tax changes, they do not bias the estimate of the elasticity, but they *do* affect the standard error. Thus, the usual two-way clustered errors are most appropriate.

5.2 Parallel Trends

There appears to be strongly suggestive evidence that home prices may be hurt by increased state income taxes. However, dynamic results for many measures of taxes call into question the parallel trends assumption necessary for correctly interpreting a difference-in-differences estimate as causal. Many specifications presented in Section 4.2 and Appendix B find that home prices are positively associated with tax cuts even two years before the tax cut in question. Thus, one might question whether the border pair partner is a suitable control group, since the two home prices begin to separate even prior to treating half of the pair with a new tax rate.

The violation of parallel trends should not be interpreted as evidence *against* the hypothesis that increased state income taxes hurt home prices. Many stories may explain such a pattern. One is that tax changes are anticipated and capitalized in home prices substantially before they take effect, in which case the causal interpretation would still be valid. On the other hand, another possibility is that secular economic conditions are specific to states, even near the border, and drive both home prices and taxes—in opposite directions; this story would invalidate any causal interpretation.

One strong exception in the dynamic analysis is that for dollar-weighted taxes, home prices do respond strongly and significantly to taxes at the expected time—immediately after a cut or hike. While the point estimates of the anticipatory responses are positive, they are generally not significant. In addition, the point estimates move sharply up, and become significant, almost immediately after the tax change. Thus, this specification satisfies parallel trends fairly well and suggests that a causal interpretation of the specification may be valid. One possible explanation for why causal effects are limited to this specification rests on the fact that dollar-weighted taxes, more than the taxes on representative middle income families, capture taxes on the wealthy, and state taxes may disproportionately affect the prices of homes owned by the wealthy. Since these homes are also the most valuable ones and are responsible for large amounts of property tax, this effect may be of utmost importance in welfare analysis.

5.3 Other Mechanisms and Confounding Factors.

I attempted to address a further concern—that the results are driven by implausible responses to tiny tax changes—using 2 event studies: An Illinois tax increase in 2011, and a Washington, D.C., tax cut in 2007. While both events involve tax changes of over 1% in dollar-weighted terms, I can only find detailed background on the Illinois case; it was unanticipated and unaccompanied by other tax changes. The Washington, D.C., case was unmentioned by the press and accompanied by tax increases in the neighboring state of Maryland.

The results from the event study exercise were mixed. Illinois results provided little evidence of the response of home prices to taxes. On the other hand, Washington, D.C., results in Figure 6 are textboook examples of event study evidence of causal effects: There are no strong pre-trends, a sharp change at the time of the event, and statistical significance after the event. The fact that the Washington, D.C., data is almost complete, and that all of the counties and ZIP codes involved are part of a single labor market, make this event an almost perfect laboratory to discuss this effect, and the fact that the results are so clear in this case is highly encouraging.

A few other concerns deserve mention. One is property taxes, another ingredient in the overall cost of living in one state versus another. Feldstein and Wrobel (1998) explicitly accounts for property taxes, but only by assuming that property values in a state are constant over the sample period, and backing out the implied millage rates from property tax collections. Since my study focuses precisely on changing property values, this method is clearly not at my disposal. However, this omission only matters to the extent that changes in property tax millage rates correlate with changes in income tax millage rates—an empirical question I do not take up here but is worthy of further investigation.

Second, these estimates, to the extent they are accepted as causal, should for some reasons be seen as upper bounds on the elasticity of home prices with respect to the state net-of-income-tax rate, and for other reasons as lower bounds. At the border, homeowners have an obvious choice when taxes in their home state rise—they can sell their homes and buy others just across the border. In a ZIP code in the center of a state, however, escaping the higher state income taxes is not so simple of a proposition; homeowners would be required to relocate to another metropolitan area and probably search for a new job. For this reason, my border estimates should be seen as upper bounds on the true elasticity across the country. On the other hand, if a worker works in one state and resides in another, he typically pays the higher of the two state income taxes. Thus, workers employed in the higher tax state but living in the lower tax state will face no incentive to relocate if their state of residence adjusts its income tax rate up or down. For this reason, my estimates should be seen as lower bounds on the true elasticity across the country.[13]

Finally, Coglianese (2015) questions the validity of border pair research designs in general. He shows that, with respect to employment rates, trends in the rest of the state are

[13]Migration is not the only mechanism by which taxes might affect home prices. For example, if everyone in the country saw their taxes increase substantially, demand would be expected to drop purely due to income effects, which would cause home prices to fall.

predictive of a county's employment situation, even after controlling for the situation in a bordering county in a neighboring state, and even in states with no change in the length of unemployment insurance benefits.

5.4 Magnitudes and MVPF Calculations

Temporarily putting aside the concerns discussed in the previous subsection, I consider whether the estimated elasticities have a plausible magnitude. To do so, I perform a back-of-the envelope calculation. For a family earning \$50,000 post-tax, a one percent drop in net-of-tax wages results in a loss of income of \$500 per year. Meanwhile, a home valued at \$500,000 and suffering a one percent drop in value results in a reduction in value of \$5,000. Assuming an interest-only loan at 5% yields a drop in annual interest payments of \$250. Thus, an elasticity around 2, as found in the empirical section, would appear to be roughly the right magnitude, as it would leave the disposable (after housing) income of the family unchanged.

Finally, I return to Equation 6, which I reproduce here,

$$MVPF = \frac{\frac{1}{\tau^L}R^L - \frac{\eta}{1-\tau^L}R^H}{\frac{1}{\tau^L}R^L - \frac{1}{1-\tau^L}\left[\eta R^H + \left(\underbrace{e_L^w + \eta e_L^H}_{\text{Total response}}\right)R^L + \left(\underbrace{e_H^w + \eta e_H^h}_{\text{Total response}}\right)R^H\right]}$$

with the goal of demonstrating the magnitude by which the MVPF may be mismeasured if the home price effect is not considered. Recall that this can be compared to the simple MVPF formula

$$MVPF = \frac{1}{1 - \frac{\tau^L}{1-\tau^L}(e_L^w + \eta e_L^h)}$$

where $e_L^w + \eta e_L^h$ is the total labor supply response to the tax change, and the house price response is ignored.

I calibrate this as follows. The state labor tax rate is around 6% (Kaeding, 2016). The ratio of state labor tax revenues to property tax revenues ranges from 4/7 to 11/7 (Malm and Kant, 2013).[14] Studies estimate the total response to federal income tax changes as having elasticities between 0.33 and 2 (Chetty et al., 2011); I assume the total response to state taxes is similar—which it would be if the labor supply response to home prices is small, a believable assumption.

With these assumptions, the MVPF using the simple formula that does not account for GE effects on home prices ranges from 1.02[15] to 1.15.[16] The properly calculated MVPF further depends on η, the elasticity estimated in this paper, and $e_H^w + \eta e_H^h$, the total response of housing consumption to the change in the labor tax rate—both the direct response to the drop in net-of-tax wages, and the indirect response to the drop in home prices—a value very difficult to estimate. I assume that the housing consumption response is nonpositive,[17] and η lies between 1 and 3, based on the results in the foregoing sections. Then a lower bound on the MVPF ranges from 1.02[18] to 1.24[19]. However, this could be substantially larger if housing consumption drops as a result of the tax. For example, if $e_H^w + \eta e_H^h = 1$—that is, a 1% reduction in the net-of-tax rate leads to a 1% reduction in amount of housing consumed—then this range shifts up, to 1.07 to 1.56. This 1.56 estimate is quite different from the upper bound of 1.15 calculated using the simple MVPF formula, and suggests that ignoring the housing price response when formulating policy is quite dangerous.

[14] First, it is unclear whether local revenue loss should be included—the agency making most decisions about labor income taxes (the state government) is different from that obtaining most revenue from property taxes (local government). However, it seems reasonable to consider local governments to be agents of the state government. Second, labor income and property taxes together account for only about 55% of state and local revenue, with most of the rest coming from sales taxes. The 4/7 figure ignores sales taxes, since in border regions they are often paid by nonresidents; the 11/7 figure counts sales taxes as labor income taxes in the public finance tradition.

[15] Labor elasticity of 0.33

[16] Labor elasticity of 2

[17] That is, I assume that the net effect of an increase in labor taxes and a decrease in home prices *does not* lead to *higher* housing consumption.

[18] Labor elasticity of 0.33, $R^L/R^H = 11/7$, $\eta = 1$

[19] Labor elasticity of 2, $R^L/R^H = 4/7$, $\eta = 3$

6 Conclusion

In summary, I find strongly suggestive but not conclusive evidence that home prices are responsive to state income taxes, with suggested estimates of the elasticity of home prices with respect to net-of-tax rates of between 1 and 2.5. The fact that some specifications are less conclusive highlights the importance of checking border pair designs for robustness using dynamic specifications and event studies. Calibrating a formula for the marginal value of public funds for state income tax adjustments shows that ignoring the effect on home prices can lead to very erroneous results, meaning that obtaining a good estimate of this elasticity is important, and worthy of further study.

I see three major directions for future work. First, one could analyze the response of home prices to other relevant taxes—for example, property taxes or local income taxes (in those jurisdictions that have them). Second, one could attempt to provide more conclusive evidence of the effect suggested by this paper by using transaction-level data. This would not directly change standard errors; the clustering at the border segment level means that more large tax change events would be required to increase precision, not merely more observations per event. However, transaction level data would allow finer observation of location, perhaps leading to a regression discontinuity approach at the state border, rather than the broader ZIP code pair approach. This would allow for a better quasi-control group, and make it more likely that conclusive causal evidence would be uncovered.

Third, in the MVPF calibration I undertook in Section 5, I was forced to make two assumptions regarding empirically estimable quantities not investigated here. First, I assumed that the labor supply response to state income taxes and federal income taxes is similar, which is equivalent to assuming that the labor supply response to lower home prices is small. Second, I assumed that the total response of housing consumption to state income taxes—both the direct response, and the countervailing response to the ensuing lower home prices—is weakly negative, and considered an overall elasticity of 0 and 1. Both of these assumptions could be empirically investigated, and the MVPF calculation could be updated with more precise values.

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A Proof of Proposition 2.1

Individual i faces the following optimization problem:

$$V(w_i(1-\tau_j^L), h_j(1+\tau_j^H)) = \max_{C,H,L} \epsilon_{ij} \left\{ C^a H^b - \frac{\theta}{1+\gamma} L^{1+\gamma} \right\}$$
$$\text{s.t.}$$
$$C + Hh_j(1+\tau_j^H) = w_i(1-\tau_j^L)L$$

First consider the problem given a certain amount of labor supplied. Then, the individual must divide his income between housing and all other consumption. Since utility over these goods is Cobb-Douglas, the income shares are fixed, meaning that

$$C = \frac{a}{a+b} L w_i (1-\tau_j^L)$$
$$H = \frac{b}{a+b} \frac{L w_i (1-\tau_j^L)}{h_j(1+\tau_j^H)}$$

Now the individual faces an unconstrained optimization over the amount of labor to supply:

$$V(w_i(1-\tau_j^L), h_j(1+\tau_j^H)) = \max_{L} \epsilon_{ij} \left\{ \frac{a^a b^b}{(a+b)^{a+b}} [h_j(1+\tau_j^H)]^{-b} [L w_i (1-\tau_j^L)]^{a+b} - \frac{\theta}{1+\gamma} L^{1+\gamma} \right\}$$

The first order condition is

$$\frac{a^a b^b}{(a+b)^{a+b}} [h_j(1+\tau_j^H)]^{-b} [L w_i (1-\tau_j^L)]^{a+b-1} = \theta L^{\gamma}$$

Rearranging yields

$$L(w(1-\tau_j^L), h_j(1+\tau_j^H)) = \tilde{L} \cdot (h_j(1+\tau_j^H))^{\frac{-b}{\gamma+1-a-b}} (w(1-\tau_j^L))^{\frac{a+b}{\gamma+1-a-b}}$$

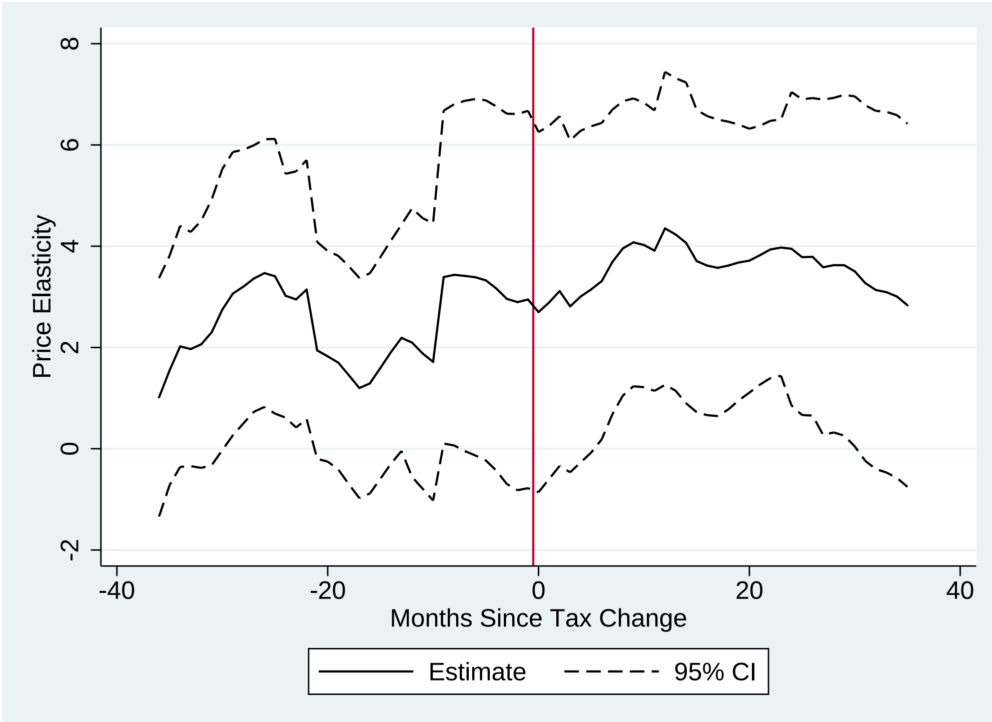
where

$$\tilde{L} = \left(\frac{a^a b^b}{(a+b)^{a+b} \theta} \right)^{\frac{1}{\gamma+1-a-b}}.$$

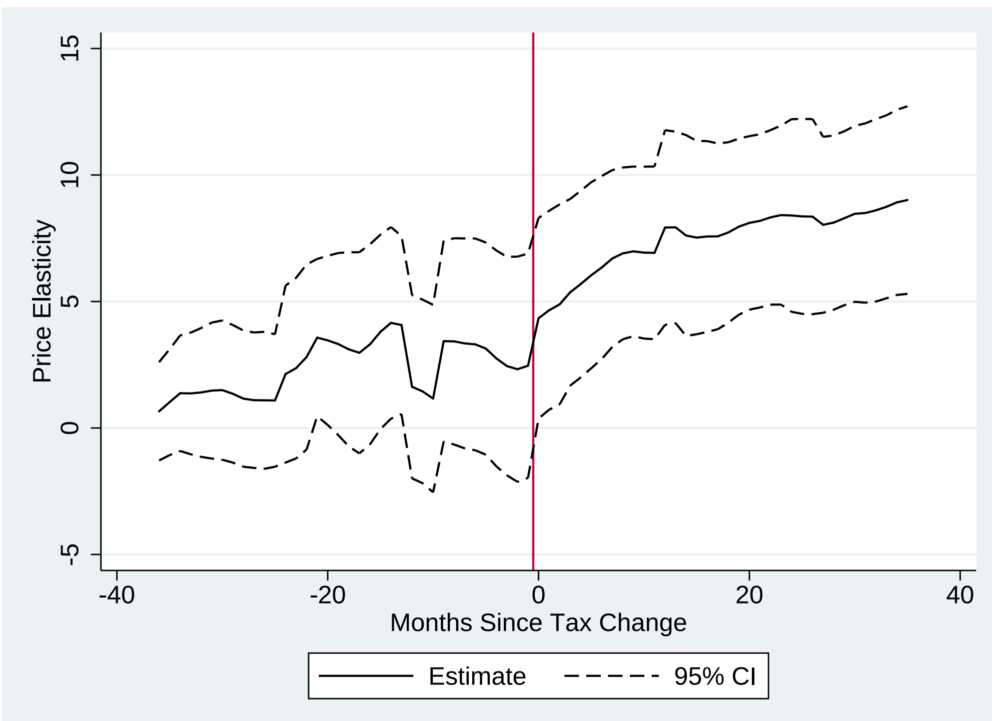
Substituting this into the expressions for C and H yields the stated forms, where $\tilde{C} = \frac{a}{a+b}\tilde{L}$ and $\tilde{H} = \frac{b}{a+b}\tilde{L}$. Substituting all of these into the utility function yields the proposed indirect utility function, where

$$\tilde{V} = \tilde{C}^a \tilde{H}^b - \frac{\theta}{1+\gamma} \tilde{L}^{1+\gamma}.$$

B Dynamic Estimates for Other Samples

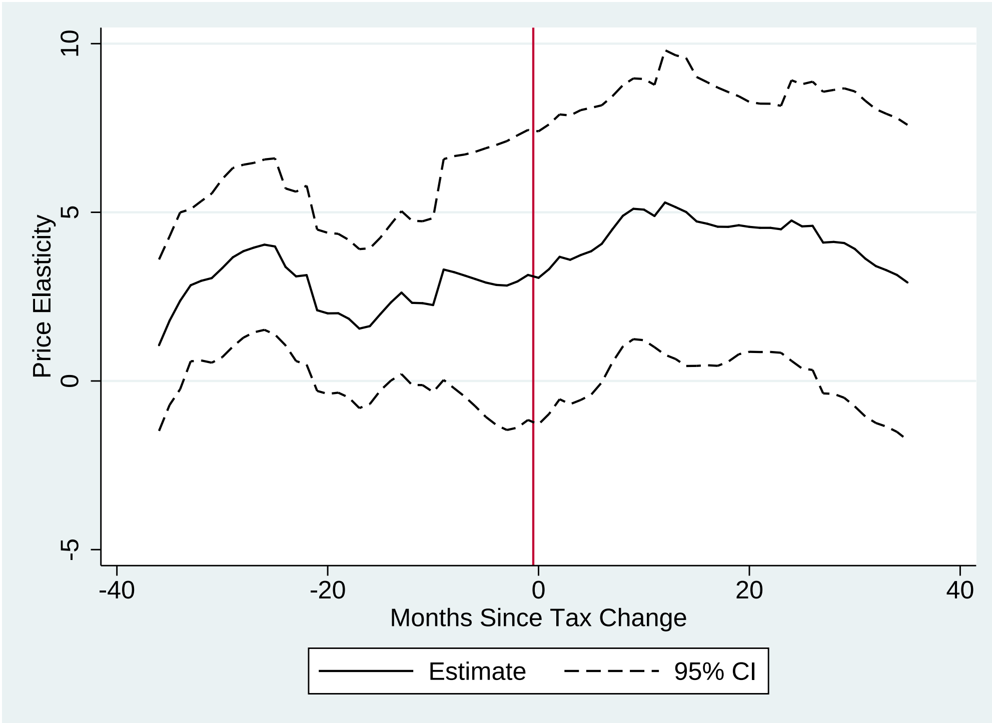


(a) ZIP codes; median home prices; taxes on families earning $50,000

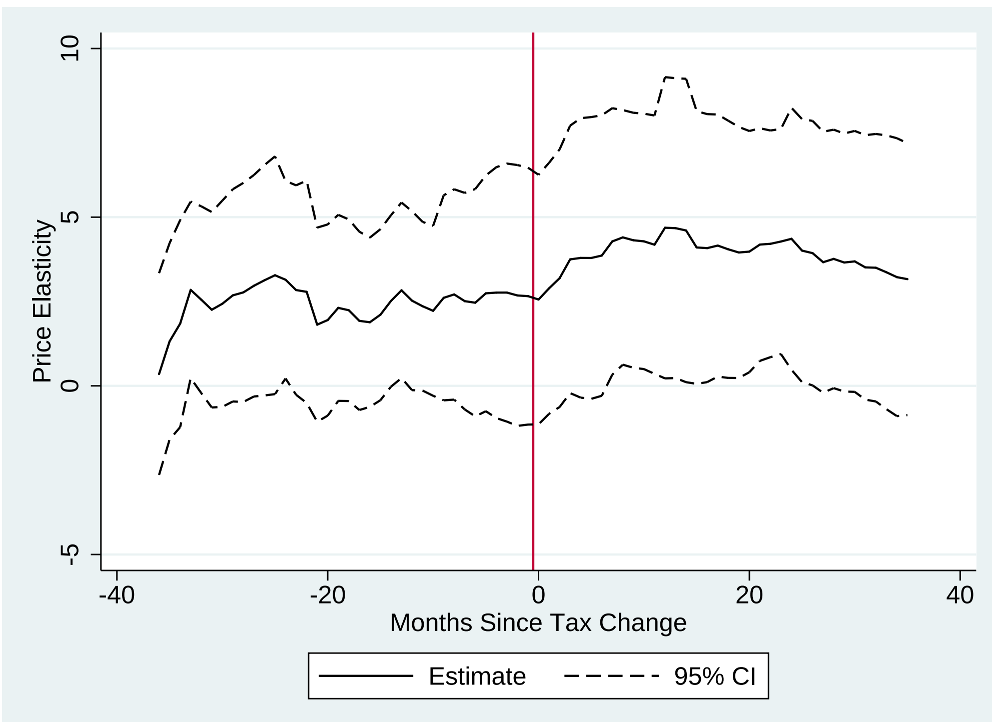


(b) ZIP codes; median home prices; dollar-weighted taxes

Figure 7: Dynamic price responses to tax changes via estimation of Equation 9. Elasticities of home prices in month $t + s$ with respect to a permanent change in the net-of-tax rate in month t are given by the solid line, with 95% confidence intervals bounded by the dashed lines. Standard errors are clustered separately at the border segment and state levels.



(c) ZIP codes; median home prices; taxes on families earning $75,000



(d) ZIP codes; per square foot home prices; taxes on families earning $75,000

Figure 7: Dynamic price responses to tax changes via estimation of Equation 9. Elasticities of home prices in month $t + s$ with respect to a permanent change in the net-of-tax rate in month t are given by the solid line, with 95% confidence intervals bounded by the dashed lines. Standard errors are clustered separately at the border segment and state levels.