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

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Abstract

I estimate the elasticity of home prices with respect to the net-of-state-income-tax rate, using three variants of a border pair strategy. I find strongly suggestive evidence that this elasticity is positive and large. However, concerns include violation of the parallel trends assumption in many specifications, the fact that most observed tax changes are small, and others. These concerns illustrate methodological issues that can arise in classic quasi-experimental research designs. Calibrating a calculation of the marginal value of public funds for state income taxes to the elasticities suggested by this study shows that ignoring such general equilibrium effects of taxes can lead to large errors in the calculation.

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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. In so doing, I make three separate contributions—one empirical, one methodological, and one theoretical: (1) Empirically, I provide suggestive causal evidence that the elasticity of home prices with respect to state income taxes is large. (2) Methodologically, I show that certain standard techniques for exploiting natural experiments to obtain causal estimates do not always succeed when investigated further. (3) Theoretically, 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 three variants of the border-county-pair approach used by Dube, Lester and Reich (2010): First, I use the simple version of the method, differencing log prices and log net-of-tax across the two members of a border pair and regressing the former on the latter, allowing for pair-fixed effects. Second, to address the issue of causation and the validity of the parallel trends assumption, I first-difference each of these spatial difference over time, and regress the monthly change in the home price differential on 3 years worth of leads and lags of monthly changes in the tax differential. Finally, to address the issue that most tax changes were too small to have believably changed home prices (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 the home price differential on pair-fixed effects and time-
fixed effects and look closely at the time-fixed effects. Though the first variant yields large and significant estimates of the elasticity of home prices with respect to the net-of-tax rate, I conclude from the other variants that this finding is suggestive rather than conclusive. This provides a strong impetus to check the results of other border studies with a dynamic specification and an event study.

The intuition behind the theoretical conclusion—that the MVPF must include the general equilibrium 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.

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 presents the specification and results for the standard border-pair difference-in-differences estimator. Section 5 lays out the analysis of the dynamic effect of state income taxes on home prices and presents the results. Section 6 carefully describes the situations surrounding the two case studies and the outcomes. Section 7 discusses the implications of these results, and ties them back to the original motivation for the paper. Finally, Section 8 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, therefore making the fiscal externality erase 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, 
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 $i \in I$, who each have the option of living in state $j = 1$ or $j = 2$. State $j$ has a linear tax rate $\tau^L_j$ on labor and $\tau^H_j$ 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

$$C_i + H_i h_j (1 + \tau^H_j) = w_i (1 - \tau^L_j) L_i.$$

Individuals differ only in the wages they earn and their idiosyncratic preference for state $j$, $\epsilon_{ij}$. 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:

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

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

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

and obtains the following indirect utility:

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

$$V(w_i(1 - \tau_j^L), h_j(1 + \tau_j^H), \epsilon_{ij}) \geq V(w_i(1 - \tau_{j'}^L), h_{j'}(1 + \tau_{j'}^H), \epsilon_{ij'})$$

$$\ln V(w_i(1 - \tau_j^L), h_j(1 + \tau_j^H), \epsilon_{ij}) \geq \ln V(w_i(1 - \tau_{j'}^L), h_{j'}(1 + \tau_{j'}^H), \epsilon_{ij'})$$

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

Suppose that the supply of housing, measured in number of housing units rather than size of house consumed, in state $j$ is $S_j$, and suppose $S_1 + S_2 = |I| \equiv 1$ 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

\footnote{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 \( F(\tilde{\epsilon}) \), then the housing market clears if and only if

\[
S_j' = F \left\{ x \left[ \ln(h_{j'}(1 + \tau_H^{j'})) - \ln(h_j(1 + \tau_H^j)) \right] + y \left[ \ln(1 - \tau_L^{j'}) - \ln(1 - \tau_L^j) \right] \right\}
\]

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 form of the empirical specifications I use throughout:

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

where

\[
\phi_{jj'} = \mathbb{E} \left\{ \frac{1}{x} F^{-1}(S_j', t) - \ln \left[ \frac{1 + \tau_H^{j'}}{1 + \tau_H^j} \right] \right\}
\]

\[
\eta = -\frac{y}{x} = \frac{a + b}{b}
\]

\[
\mu_{tjj'} = \frac{1}{x} \left\{ F^{-1}(S_j', t) - \mathbb{E} F^{-1}(S_j', t) \right\} - \left\{ \ln \left[ \frac{1 + \tau_H^{j'}}{1 + \tau_H^j} \right] - \mathbb{E} \ln \left[ \frac{1 + \tau_H^{j'}}{1 + \tau_H^j} \right] \right\}
\]

Estimating this equation—especially \( \eta \)—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 \( \epsilon \). 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. 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 \( \frac{\epsilon}{1 - \tau_j^L} \eta h_{jt} H(w_i(1 - \tau_j^L), h_j(1 + \tau_j^H)) \).

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{align*}
\text{Labor Supply to Wage} & \quad - \frac{\epsilon}{1 - \tau^L} e^w_L w_i \tau^L_j L(w_i(1 - \tau^L_j), h_j(1 + \tau^H_j)) \\
\text{Labor Supply to Housing Prices} & \quad - \frac{\epsilon}{1 - \tau^L} \eta e^h_L w_i \tau^L_j L(w_i(1 - \tau^L_j), h_j(1 + \tau^H_j)) \\
\text{Housing Consumption to Wage} & \quad - \frac{\epsilon}{1 - \tau^L} e^w_H h_j \tau^H_j H(w_i(1 - \tau^L_j), h_j(1 + \tau^H_j)) \\
\text{Housing Consumption to Housing Prices} & \quad - \frac{\epsilon}{1 - \tau^L} \eta e^h_H h_j \tau^H_j H(w_i(1 - \tau^L_j), h_j(1 + \tau^H_j))
\end{align*}

where $e^w_L$ is the (direct) elasticity of labor supply with respect to net-of-tax wage, and other $e$s are defined similarly.

Notice that we can replace $w_i \tau^L_j L_i$ with $R^L_i$, the revenue from the labor tax collected from individual $i$ and $h_j \tau^H_j H_i$ with $R^H_i$, 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

\[
MVPF = \frac{1}{\tau^L} \frac{R^L - \eta R^H}{1 - \frac{\eta}{\tau^L} R^H} \left[ \eta R^H + \left( \frac{e^w_L + \eta e^h_L}{\text{Total response}} \right) R^L + \left( \frac{e^w_H + \eta e^h_H}{\text{Total response}} \right) R^H \right]
\]

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

\[
MVPF = \frac{1}{1 - \frac{\tau^L}{1 - \tau^L} (e^w_L + \eta e^h_L)}
\]

which is the standard form.

After empirically estimating $\eta$ in the ensuing sections, I will return to this MVPF calculation in Section 7.
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 2010), location, area, and adjacency of all U.S. counties was taken from the 2010 Census Gazetteer Files; the Files identify 3,234 counties in the U.S. 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. In the case of both counties and ZIP codes, I define a border pair as a pair of counties or 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 median 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 on the median home prices and the median value per square foot. These data are monthly from April 1996 through the present, though not every county or ZIP code has data in any given month.

\footnote{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 shows the availability of median home price data by county and ZIP code. While most 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. Data coverage is especially good for the border counties, with over 70% of the residents of border counties covered by 1997, and over 80% by 2007. Data coverage is relatively poor for the border ZIP codes, but even then, over 50% of that population is covered for the entire sample period, and over 60% by 2006. 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.

Summary statistics for the Zillow data can be found in Table 1. As in Dube, Lester and Reich (2010), I present summary statistics from the home price data using two samples: the entire sample of all counties or ZIP codes for which data is available, and the sample that includes only those counties or 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 are large, with even the smallest sample (1997 data on counties in a border pair) containing 197 counties. Additionally, the data appears to be fairly representative, with the values at least qualitatively in line with national home prices.
Figure 2: Population covered by Zillow data. In each month, the 2010 populations of the counties which Zillow covers in that month are summed and divided by the sum of the 2010 populations of all counties to arrive at the point on the graph. (Likewise, except using 2014 populations, for ZIP codes.) Thus, the upward trend in the graph is due only to the increasing quality of data, and not to population growth.
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.

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<td>1019</td>
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<td>24</td>
<td>1093</td>
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<td>81</td>
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<td><strong>Counties in Border Pair</strong></td>
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<tr>
<td>Median Value</td>
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<td>109348</td>
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<td>265</td>
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<td>121557</td>
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<tr>
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<tr>
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<td>72</td>
<td>26</td>
<td>717</td>
<td>138</td>
<td>105</td>
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</table>

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, 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

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\footnote{In a few states, federal tax is deductible on the state tax return, but this is accounted for in the data.}
interpreted as elasticities), we have \( \log(1 - \tau_T) = \log(1 - \tau_f) + \log(1 - \tau_s) \). Differencing across two states, which I do in all of my specifications, means that inclusion of the \( \log(1 - \tau_f) \) term does not matter.

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. 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. 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.

\(^6\)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.
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.

<table>
<thead>
<tr>
<th></th>
<th>1997</th>
<th>2010</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>count</td>
<td>mean</td>
</tr>
<tr>
<td><strong>Levels</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Family of 4 with $50,000 Income</td>
<td>51</td>
<td>2.983</td>
</tr>
<tr>
<td>Family of 4 with $75,000 Income</td>
<td>51</td>
<td>3.521</td>
</tr>
<tr>
<td>Dollar-Weighted</td>
<td>51</td>
<td>3.106</td>
</tr>
<tr>
<td><strong>Year-Over-Year Changes, All Years Since 1997</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Family of 4 with $50,000 Income</td>
<td>663</td>
<td>-0.0479</td>
</tr>
<tr>
<td>Family of 4 with $75,000 Income</td>
<td>663</td>
<td>-0.0370</td>
</tr>
<tr>
<td>Dollar-Weighted</td>
<td>714</td>
<td>0.00303</td>
</tr>
</tbody>
</table>

4 Simple Difference-in-Differences

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

\[
\ln \left[ \frac{h_{j't}}{h_{j't}} \right] = \phi_{jj'} + \eta \ln \left[ \frac{1 - \tau_{Lj't}}{1 - \tau_{Lj't}} \right] + \mu_{tjj'}
\]

where \(j\) and \(j'\) are two regions, \(h_{j't}\) is the home price in region \(j\) at time \(t\), \(\tau_{Lj't}\) is the labor tax rate in region \(j\) at time \(t\), and

\[
\mu_{tjj'} = \frac{1}{x} \left\{ F^{-1}(S_{j't}, t) - E F^{-1}(S_{j't}, t) \right\} - \left\{ \ln \left[ \frac{1 + \tau_{Hj't}}{1 + \tau_{Hj't}} \right] - E \ln \left[ \frac{1 + \tau_{Hj't}}{1 + \tau_{Hj't}} \right] \right\}
\]

is the error term.

This equation, derived from the structural model of Section 2.1, can be estimated directly by ordinary least squares iff \(\ln \left[ \frac{1 - \tau_{Lj't}}{1 - \tau_{Lj't}} \right]\) is uncorrelated with \(\mu_{tjj'}\) within a given pair of regions \(j\) and \(j'\). \(\mu_{tjj'}\) 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 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. Specifically, 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. A simple empirical strategy, such as estimating the following panel fixed effects model

\[
\ln h_{jt} = \phi_j + \psi_t + \eta \ln(1 - \tau_{jt}) + \mu_{jt},
\]

or its difference across all pairs of regions, will 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. Counties or ZIP codes that are neighboring, but in different states, should experience similar shocks to their pure desirabilities, meaning relative preferences should not change much.

The classic border pair specification proposed in [Dube, Lester and Reich (2010)] is

\[
\ln h_{jpt} = \phi_j + \psi_{pt} + \eta \log(1 - \tau_{jpt}) + \mu_{jpt},
\]

where \( p \) indexes the border pair and \( \psi_{pt} \) is a pair-time fixed effect. However, especially for the ZIP code sample, this specification requires estimating a substantial number of nuisance parameters \( \psi_{pt} \). Additionally, Equation 5 shows that it is the difference between the two regions in the pair that is relevant. Thus, I difference Equation 7 between the two regions within the pair to obtain the following:

\[
Dh_{pt} = \phi_p + \eta D \log(1 - \tau)_{pt} + \mu_{pt},
\]
where I have replaced $\phi_i - \phi_j$ with $\phi_p$. Relative to Equation 7, this is far less computationally demanding and, if anything, will bias $\hat{\eta}$ toward 0, since it relaxes the requirement that the region $i$ fixed effect be constant across the pairs of which it is a member.

The residuals are subject to various types of correlation across observations as noted by Dube, Lester and Reich (2010). First, there is serial autocorrelation at the region level, which implies there is serial autocorrelation at the pair level. In addition, pairs along a border segment (pair of neighboring states) may be correlated mechanically, since the same region will appear multiple times—one for each bordering region that is in the other state. However, following Dube, Lester and Reich (2010) and using standard errors that are clustered at the border segments level produces very large standard errors (greater than 1), thereby making meaningful inference very difficult. Of course, this isn’t that surprising, since there are only about 60 clusters.

An alternative that corrects for residuals that are serially correlated as well as arbitrary cross-sectional spatial dependence is Driscoll and Kraay (1998) standard errors. The main weakness for these standard errors compared to clustering at the border segments level is that these are not robust to residuals that are arbitrarily serially correlated, but only those that are related in a moving average form. I allow for up to a 36-month lag in this serial correlation. In other ways, though, these standard errors are more robust in that they allow for arbitrary forms of cross-sectional spatial dependence, not only within a given border segment.

Figure 3 shows binscatters of median price differences within a border pair versus net-of-tax differences for families earning $50,000, controlling for pair fixed effects. The relationship appears to be strong. Table 3 presents the regression results. Point estimates of the elasticity of home prices with respect to the net-of-tax rate range from 1.464 to 3.218 across these specifications, all of which are large. As mentioned above, standard errors obtained by

---

7 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.

8 There is no need to use Cameron, Gelbach and Miller (2011) clustering as in Dube, Lester and Reich (2010), because I have differenced within the pair.
Table 3: Basic difference-in-differences estimates. All dependent and independent variables are differences of logs within each border pair, which is the unit of observation. Coefficients should be interpreted as elasticities. All independent variables are (differences of logs of) average state net-of-tax rates on the population listed. All models include pair fixed effects. Standard errors, clustered at the border segment (pair of states) level, are in parentheses. Driscoll and Kraay (1998) standard errors are in brackets.

<table>
<thead>
<tr>
<th>Counties</th>
<th>Median Home Price</th>
<th>Per Sq. Ft. Home Price</th>
</tr>
</thead>
<tbody>
<tr>
<td>Family Earning $50k</td>
<td>3.218</td>
<td>3.039</td>
</tr>
<tr>
<td></td>
<td>(1.798)</td>
<td>(1.750)</td>
</tr>
<tr>
<td></td>
<td>[0.488]</td>
<td>[0.390]</td>
</tr>
<tr>
<td>Family Earning $75k</td>
<td>3.121</td>
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</tr>
<tr>
<td></td>
<td>(1.994)</td>
<td>(1.971)</td>
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<tr>
<td></td>
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<td>Dollar-Weighted</td>
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<td></td>
<td>(1.726)</td>
<td>(1.679)</td>
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<tr>
<td></td>
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<tr>
<td>Observations</td>
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<tr>
<td></td>
<td>38460</td>
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<table>
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<th>ZIP Codes</th>
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<tr>
<td></td>
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<td></td>
<td>[0.567]</td>
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<tr>
<td>Family Earning $75k</td>
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<td></td>
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<td>107103</td>
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</table>
Figure 3: Median price differences vs. net-of-tax differences for families earning $50,000. Both variables have had logs taken before differencing, meaning the slope should be interpreted as an elasticity. The specification includes pair fixed effects.

clustering at the border segment level are quite large—usually at least close to 1—making minimum detectable effects also quite large. Using Driscoll and Kraay (1998) standard errors reduces the confidence intervals drastically and makes all of the estimates significant at the 1% level.

5 Dynamic Response

The identification assumption necessary for the validity of the strategy in Section 4 is commonly known as parallel trends. That is, suppose county $i$ is a member of one state that changes its tax rate from year $t$ to year $t+1$, and county $j$ is a neighboring member of another state that does not. Equation 8 considers county $j$ to be a control group for county $i$. The pair-fixed effect controls for the possibility that county $j$ may systematically have higher or lower house prices than county $i$, but only if the gap between them would be expected to have been constant in the absence of the tax change. In the language of Section

\footnote{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.}
refsubsec:struct, 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

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

iff $s \geq 0$. That is, the effect of a tax change should only be felt on or after the date of its passage.\(^{10}\)

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

$$D \ln h_{pt} = \phi_p + \sum_{s=-(T-1)}^T (\eta_s \Delta D \ln(1-\tau)_{p,t+s}) + \eta_T D \ln(1-\tau)_{p,t-T} + \mu_{pt}. \quad (9)$$

As they note, specifying the tax variables in first-differences over time allows estimation of elasticities $\eta_s$ 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 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) twelve ways: median and per square foot prices as the dependent variable; counties and ZIP codes as the unit of observation; and the three tax measures as the independent variable. Having shown the methodological problems with clustered standard errors, I will use only Driscoll and Kraay (1998) standard errors. Figure 4 visualizes

\(^{10}\)or anticipated passage. I will return to this idea later.
these estimates with the ZIP code sample and per square foot prices. Other samples yield qualitatively similar results and can be found in Appendix B. In general, these elasticities are positive, large, and significant at many leads and lags, except in the case of dollar-weighted taxes. This suggests that parallel trends is unlikely to be satisfied in this study, at least in many specifications. I will discuss possible explanations and implications of this in Section 8.

6 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. Even a sizable elasticity like 5, estimated in Section 5, 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.

- 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, 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

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11Section 5 suggests dollar-weighted taxes cause the most response.
Figure 4: Dynamic price responses to tax changes. 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. I use Driscoll and Kraay (1998) standard errors allowing serial autocorrelation of residuals up to 36 months.
Figure 4: Dynamic price responses to tax changes. 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. I use Driscoll and Kraay (1998) standard errors allowing serial autocorrelation of residuals up to 36 months.
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 (Feenberg and Coutts 1993) 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 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.

The simplest way to do this event study would be to simply take the mean, over all border pairs, of the log price difference (subtracting the neighboring county/ZIP code from the one in the state in question) at each month, with error bars, and plot them over time.
However, since my panel of pairs is unbalanced, this strategy is vulnerable to the possibility that pairs entering or leaving my sample are driving the results.

To address this issue, I will use regression with pair-fixed effects:

\[
D \ln h_{pt} = \phi_p + \psi_t + \mu_{pt},
\]

where all differences will be arranged such that I subtract the bordering state from the state being studied. I do not control for occurrences in the neighboring states, since these episodes have been selected because they are substantially larger in magnitude than other tax changes. The time fixed effect for the month at which the event took place will be omitted, so all time fixed effects will show the gap in home prices relative to the gap in home prices in the month at which the event took place.

Since pairs that share a county will have mechanically correlated residuals, standard errors should be clustered at the level of the “home” county/ZIP code and neighboring county/ZIP code separately (Cameron, Gelbach and Miller [2011]). In addition, this corrects for arbitrary serial correlation of residuals. However, the number of clusters is a concern, especially for the counties sample. For example, in the Illinois study, there are only 10 Illinois counties and 14 neighboring counties. Thus, I will not cluster the standard errors and will only make them robust to heteroskedasticity.

Figure 5 depicts results for Illinois, and Figure 6 depicts results for Washington, D.C. Turning first to Illinois, home prices appear to have been declining (relative to neighbors) for several years prior to 2011, invalidating the parallel trends assumption. That said, 95% confidence intervals of the (normalized) difference almost always include zero in the pre-period, but often exclude zero in the post-period, providing suggestive evidence that the taxes may have induced a fall in home prices. In ZIP code level data, the results are better; relative median home prices didn’t appear to begin to fall until 2010, when the state’s fiscal position might have been quite forecastable, and relative per square foot home prices appear
Figure 5: Illinois event study. The percentage by which Illinois home values exceed values in neighboring counties in month $t$ 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 robust to heteroskedasticity. Regressions feature 5,071 observations over 22 county pairs.
Figure 5: Illinois event study. The percentage by which Illinois home values exceed values in neighboring ZIP codes in month $t$ 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 robust to heteroskedasticity. Regressions feature 10,535 observations over 67 ZIP code pairs.
Figure 6: Washington, D.C., event study. The percentage by which Washington, D.C., home values exceed values in neighboring counties in month $t$ 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. Standard errors are robust to heteroskedasticity. Regressions feature 1,085 observations over 5 county pairs.
Figure 6: Washington, D.C., event study. The percentage by which Washington, D.C., home values exceed values in neighboring ZIP codes in month $t$ 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. Standard errors are robust to heteroskedasticity. Regressions feature 7,106 observations over 34 ZIP code pairs.
to be flat in the pre-period.

The results for Washington, D.C., look different. Though the standard errors for the county sample are prohibitively large due to the presence of only 5 county pairs (the 5 bordering counties, and the single county in D.C.), the results for the ZIP code sample are quite clear: 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\textsuperscript{12}—and highly significant.

7 Discussion

7.1 Causal Concerns

There appears to be strongly suggestive evidence that home prices may be hurt by increased state income taxes. The basic difference-in-differences estimate of the elasticity of home prices with respect to the net-of-tax rate, from Section 4, is large in magnitude (ranging from 1.46 to 3.22) and highly significant.

On the other hand, the dynamic analysis of Section 5 calls into question the parallel trends assumption necessary for correctly interpreting a difference-in-differences estimate as causal. Most specifications presented there and in Appendix B find that home prices are positively associated with tax cuts even two years before the tax cut in question. Most of these responses do rise for positive lags; that is, home prices are more strongly associated with past tax changes than future tax changes. However, this is equivalent to a triple difference estimate, which is usually used very cautiously.

The existence of pre-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 \textsuperscript{12}although the reader should keep in mind the simultaneous tax increases in Maryland
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 the response of home prices at the ZIP code level to dollar-weighted taxes. 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 ZIP code level median home prices, more than county level median home prices, capture changes in the values of expensive homes. Therefore, state taxes may disproportionately affect the prices of homes owned by the wealthy. However, 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.

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 mostly supportive of the hypothesis that state income taxes negatively affect home prices. Illinois results had pre-trends in the point estimates, but generally zero lay in the 95% confidence interval for the pre-period but not the post period. On the other hand, Washington, D.C., results in Figure 6 are textbook
examples of event study evidence of causal effects: There are no strong pre-trends, a strong 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.

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 predictive of a county’s employment situation, even after controlling for the situation in a bordering county in a neighboring state.

7.2 MVPF Calculations

Temporarily putting aside the concerns discussed in the previous subsection, I return to Equation 6, which I reproduce here,

\[
MVPF = \frac{1}{\frac{1}{\tau_L} R_L - \frac{\eta}{1 - \tau_L} R_H} \left[ \frac{\frac{1}{\tau_L} R_L - \frac{\eta}{1 - \tau_L} R_H}{\frac{1}{\tau_L} R_L - \frac{1}{1 - \tau_L}} \right] \left[ \frac{\eta R_H}{\left( e^w_L + \eta e^h_L \right) R_L} + \left( e^w_H + \eta e^h_H \right) R_H \right]
\]

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

\[
MVPF = \frac{1}{1 - \frac{\tau_L}{1 - \tau_L} (e^w_L + \eta e^h_L)}
\]

where \(e^w_L + \eta e^h_L\) 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). 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.

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.
With these assumptions, the MVPF using the simple formula ranges from 1.02\textsuperscript{14} to 1.15\textsuperscript{15}. The properly calculated MVPF further depends on $\eta$, the elasticity estimated in this paper, and $e^w_H + \eta e^h_H$, 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\textsuperscript{16} and $\eta$ lies between 1 and 3, based on the results in the foregoing sections. Then a lower bound on the MVPF ranges from 1.02\textsuperscript{17} to 1.24\textsuperscript{18}. However, this could be substantially larger if housing consumption drops as a result of the tax. For example, if $e^w_H + \eta e^h_H = 1$—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.

8 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 3. 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.

\textsuperscript{14}Labor elasticity of 0.33  
\textsuperscript{15}Labor elasticity of 2  
\textsuperscript{16}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.  
\textsuperscript{17}Labor elasticity of 0.33, $R^L/R^H = 11/7$, $\eta = 1$  
\textsuperscript{18}Labor elasticity of 2, $R^L/R^H = 4/7$, $\eta = 3$
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 decrease standard errors but, more importantly, 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 7, 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.

References


Davis, Aaron C. 2014. “D.C. Council backs first big tax cut in 15 years; city aims to be more competitive with Md., Va.”


A Proof of Proposition 2.1

Individual $i$ faces the following optimization problem:

$$V(w_i(1 - \tau^L_j), h_j(1 + \tau^H_j)) = \max_{C,H,L} \epsilon_{ij} \left\{ C^a H^b - \frac{\theta}{1 + \gamma} L^{1+\gamma} \right\}$$

s.t.

$$C + H h_j(1 + \tau^H_j) = w_i(1 - \tau^L_j)L$$

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

$$C = \frac{a}{a+b} L w_i(1 - \tau^L_j)$$

$$H = \frac{b}{a+b} h_j(1 + \tau^H_j)$$

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

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

The first order condition is

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

Rearranging yields

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

where

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

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
Figure 7: Dynamic price responses to tax changes. 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. I use Driscoll and Kraay (1998) standard errors allowing serial autocorrelation of residuals up to 36 months.
Figure 7: Dynamic price responses to tax changes. 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. I use Driscoll and Kraay (1998) standard errors allowing serial autocorrelation of residuals up to 36 months.
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