

# HOME-EQUITY LENDING AND RETAIL SPENDING: EVIDENCE FROM A NATURAL EXPERIMENT IN TEXAS

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## **Abstract**

This paper estimates the importance of credit-constraints from a ‘natural experiment’ – a 1997 Texas constitutional amendment that relaxed stringent constraints on the ability of homeowners to borrow using home-equity as collateral. If credit constraints bind, then such an increase in credit availability is likely to have a positive effect on household spending. We investigate this possibility using retail sales data at the county and state level. Using difference-in-difference methods, we find that Texas households increased retail expenditures from before to after the passage of the amendment, relative to the change in spending by non-Texas households, by magnitudes ranging from 4% to 15%.

**Keywords:** Difference-in-difference, credit-constraints, liquidity-constraints

**JEL Codes:**

# 1 Introduction

The importance of credit-constraints to the overall economy remains an important and unsettled empirical question, despite numerous studies on the subject (e.g., Jappelli 1990, Gross and Souleles 2002, Leth-Petersen 2010). Households facing binding constraints on their ability to borrow on future income – either through pure quantity restrictions or by facing relatively high borrowing rates – will be unable to optimally smooth consumption intertemporally. Such constraints therefore distort behavior and reduce welfare. Their importance is relevant for modelling and understanding aggregate consumer behavior (Campbell and Mankiw 1989), the role of financial market frictions in driving fluctuations in output and asset prices (Kiyotaki and Moore 1997), the effects of fiscal and tax policies (Agarwal, Liu and Souleles 2007, Souleles 1999), saving and economic growth (Japelli and Pagano 1999), the spillover of housing markets on overall economic activity (Iacoviello 2004, 2005), and many other issues.

The most likely causes of credit-constraints are the adverse selection and moral hazard problems arising from asymmetric information, the fact that potential borrowers know more about the likelihood of repayment than potential lenders. Collateralized debt is a common means of resolving, or at least mitigating, these information costs. The ability of homeowners to secure debt based on the value of the equity in their home, the most important source of collateral for most individuals,<sup>1</sup> can significantly reduce information costs and the financial market frictions they cause, and thus promote the efficient flow of funds through these markets. We would expect, then, legal restrictions on home-equity lending to increase information costs and tighten borrowing constraints, at least for some households.

Prior to 1998, homeowners in Texas faced such legal restrictions on home-equity loans. Until then, the Texas state constitution effectively prohibited the use of home-equity as collateral on loans except for a very limited set of expenditures, primarily home-improvement

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<sup>1</sup>According to flow of funds data, housing wealth comprises about half of total household wealth in the U.S. About two-thirds of U.S. households are homeowners.

spending and tax payments. But in a flurry of activity from 1997 to 1999, Texas citizens amended their constitution to allow home-equity lending for general spending purposes. At the time the amendment passed, no other states in the US restricted home-equity loans from being used for discretionary household spending as severely as Texas.

In this paper, we use the unique timing of this event in Texas as a ‘natural experiment’ to estimate the response of household spending to an exogenous increase in the availability of credit, with the aim of quantifying the importance of credit-constraints. If homeowners are not liquidity-constrained (and do not expect to be constrained in the future), we would not expect overall spending to respond to an unanticipated increase in the availability of credit (although such households might use new debt to pay down other higher-cost debt or convert home-equity into other assets). On the other hand, liquidity-constrained households spend less on current consumption than is optimal given their lifetime budget constraints, so their spending will likely rise when borrowing restrictions are lifted. And even if households are not currently constrained to borrow, anticipation of future constraints can limit current spending and increase saving through precautionary motives.<sup>2</sup> If we assume the amendment’s approval was generally unanticipated, we can disentangle the causal effects of credit availability from other sources of credit market shocks.

Most recent studies on the importance of liquidity constraints rely on individual-level data. For example, Gross and Souleles (2002) estimate the response of credit card-holder debt to exogenous increases in credit-card limits, Hurst and Stafford (2004) determine the response of spending to negative income (unemployment) shocks, Yamashita (2007) examines the propensity for households to take on home-equity debt when house prices rise, Stephens (2008) estimates the response of consumption to the predictable increase in discretionary income upon the pay-off of vehicle loans, and Agarwal, Liu and Souleles (2007) estimate spending responses to temporary tax rebates. Each of these studies finds evidence for the existence of credit-constraints. Most closely related to our work in terms of the nature of

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<sup>2</sup>See, for example, Zeldes (1989).

the exogenous event is the recent study by Leth-Petersen (2010), which reports estimates of individual consumption and debt responses to a 1992 credit market reform in Denmark that, as with the Texas amendment, freed homeowners from constraints on the use mortgage loans for general spending purposes. His work generally finds evidence of behavior consistent with binding credit-constraints, although the magnitude of the estimated effects is not large.

Our paper differs from these studies in important ways. Most obviously, we examine the implications of a different event – the Texas constitutional amendment on home-equity loans – which to our knowledge has not yet been examined in the context of liquidity-constraints. Not only does this episode add a new data point to extant work, it allows us, by the nature of the experiment, to directly observe the relevant behavior of the ‘treated’ group (Texas households) and the ‘control’ group (non-Texas households). Leth-Petersen (2010), for example, must make theoretical assumptions regarding which sets of households will be affected by the credit market reform (in particular, ‘illiquid’ households). In addition, our dependent variable is not individual or household-level spending, but spending aggregated at the county and state levels. This level of aggregation allows us directly to estimate the effects on variables of ultimate importance for policy, and to better relate our work to studies on the determinants of aggregate consumption. Leth-Petersen, for example, must infer aggregate effects that are implied by his individual-level results (2010, p. 1101).<sup>3</sup>

Our interest in this topic arose from our earlier work on the role of housing in the aggregate economy, and in particular the mechanisms by which fluctuations in house prices affect non-housing spending (if at all). The conventional view that declines in house prices make homeowners feel poorer and therefore reduce spending via a pure ‘wealth effect’ is theoretically flawed, and has little empirical support. Recent empirical research, however, suggests that house prices can matter for overall spending for the reasons we’ve noted here, namely, the collateral effect on cash-constrained households (e.g., Iacoviello 2004, 2005 and

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<sup>3</sup>Nonetheless, using individual-level data to estimate the effects of the Texas amendment is also important; we are currently working on this project.

Nehri and Iacoviello 2010). Here we exploit an exogenous shock that is unlikely to directly cause a wealth effect, but is very likely related to a collateral-shock. We see our results as contributing to this debate.

We use retail sales as a proxy for non-housing spending. Clearly, this measure is an imperfect proxy for overall (non-housing) consumption. For example, some spending by Texans will be on goods sold outside the state, and some retail sales in Texas will be to non-Texans. As well, important categories of spending and consumption are ignored. Nonetheless, we expect retail sales to be a reasonable proxy for non-housing consumption, and how sales responded to the policy change is of interest in its own right. In any case, other aggregate spending data at the county and state level are difficult to come by. Because we observe retail sales in Texas and outside of Texas, and before and after the amendment, we use a simple difference-in-difference model to estimate the spending effects of the Texas constitutional amendment ‘treatment,’ as we describe in section 3 below.

As discussed in section 4 of our paper, we find strong support for the importance of credit constraints: retail sales in Texas – county-wide and state-wide – increase substantially in response to the lifting of home-equity constraints. Our difference-in-difference estimates suggest an increase in spending attributable to the Texas constitutional amendment in the range of 4% to 15%, depending on data and model specification.

## **2 The Texas Constitutional Amendment**

The original restrictions on mortgage lending in Texas date back to before the territory’s establishment as a US state in 1845. The Texas Homestead Act of 1839, passed in response to the Banking Panic of 1837 and intended to protect citizens from forced sales of their property, prohibited lenders from imposing a lien on homesteads except for the original purchase of the home, to pay liabilities incurred in a divorce proceeding, to pay off federal tax liabilities, or to make home improvements. These limitations were incorporated into the original state

constitution in 1845, and remained in force until 1998. *House Joint Resolution 31*, passed by the Texas House and Senate in May 1997, proposed to amend the constitution to expand “the types of liens for home equity loans that a lender, with the homeowner’s consent, may place against a homestead.” In effect, the amendment allowed homeowners in Texas to borrow on their home equity through traditional home equity loans (up to a total loan-to-fair-market-value of 80%) and ‘reverse mortgages’,<sup>4</sup> for any type of spending they desired.<sup>5</sup> Texas voters approved this resolution on November 4, 1997, with the resulting changes effective January 1, 1998. However, uncertainties in the amendment regarding both traditional home-equity loans and reverse mortgages potentially hindered home-equity lending in the early years. These uncertainties were resolved with another constitutional amendment passed in November 1999.<sup>6</sup> At the time of these events, no other state in the nation imposed such severe restrictions on the use of home-equity loans.

What was the potential for home-equity lending in Texas at the time of this legislative activity? Contemporary accounts and reports estimate home-equity in the state during the 1990’s to have been around \$120 billion to \$200 billion, with the potential for collateralized lending estimated to range from \$4 billion to \$10 billion annually (Skelton 1997, p. 4). According to Census data, the total nominal value of specified owner-occupied housing units in Texas in 2000 was \$318 billion,<sup>7</sup> so these values seem reasonable.

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<sup>4</sup>Reverse mortgages, typically used by the house-rich but cash-poor elderly, allow borrowers to receive fixed monthly payments over a given period in return for sacrificing equity in the home at the time of the owner’s death or the home is sold or vacated.

<sup>5</sup>The proposal amends Section 50, Article XVI of the constitution, in part, as follows (added section in *italics*): “The homestead of a family, or of a single adult person, shall be, and is hereby protected from forced sale, for the payment of all debts except for ... *an extension of credit that is secured by a voluntary lien on the homestead created under a written agreement with the consent of each owner and each owner’s spouse...*”

<sup>6</sup>The 1999 amendments redefined reverse mortgages to be consistent with federal rules, and altered the required size of urban homesteads to enhance eligibility for home-equity loans. While these amendments relaxed restrictions on traditional home-equity loans, they did not permit home-equity lines of credit; these were not made legal until yet another constitutional amendment passed in 2003. For details of all of these legislative developments, see *Home Equity, Reverse Mortgage, and Home Improvement Lending in Texas: Information and Resources*, <http://www.fc.state.tx.us/homeinfo/homeindex.htm>, Finance Commission of Texas.

<sup>7</sup>Bennefield (2000). ‘Specified owner-occupied housing units are “owner-occupied single-family homes on less than 10 acres without a business or medical office on the property.” In the nation, such housing represents

Since the passage of the amendment, the Office of Consumer Credit Commissioner in Texas has collected institutional survey data on the extent of home-equity lending in the state. Because the surveys are exclusively for licensed lenders and report the value of lending by the respondents only, their data provide a lower bound on home-equity lending. In 1998 according to these data, licensed lenders made 217,163 first- and second-lien home-equity loans, for a total value loaned of \$16.2 billion. In 1999, the number of loans rose to 252,874 and the value rose to \$17.9 billion. Home-equity lending fell to 188,871 loans valued at \$10.5 billion in 2000, but rose once again in 2001 to \$21.8 billion.<sup>8</sup> Actual home-equity lending in the initial years after the amendment thus appear to have exceeded the earlier projections. [Add percentage of households taking out home-equity loans.]

### 3 Empirical methods

#### 3.1 Empirical model

We have panel data on county-wide and state-wide retail sales, observed both before and after the passage of the Texas constitutional amendment. We use a difference-in-difference model to estimate the treatment effects of this natural experiment.<sup>9</sup> Because those being treated – Texas citizens – are not likely have selected into the experiment, a matching estimator is not necessary to control for sample selection bias.

Let  $c_{it}$  denote (log) real retail sales per capita for cross-sectional unit  $i = 1, \dots, n$  (alternatively, county or state) at time  $t = 1, \dots, T$ . Our general difference-in-difference specification

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almost half of all housing stock. In Texas, the number of these housing units in 2000 was 3,849,585; the total housing stock in the state in July of that year was estimated to be 8,201,714 (Annual Estimates of Housing Units for the United States and States: April 1, 2000 to July 1, 2009 (HU-EST2009-01), U.S. Census Bureau, Population Division, June 2010. <http://www.census.gov/popest/housing/HU-EST2009.html>.)

<sup>8</sup>Texas Finance Commission. <http://www.fc.state.tx.us/homeinfo/herpty01.pdf>

<sup>9</sup>Much of our thinking on the empirical model has been guided by the work of Cornwell and Mustard (2007).

is

$$c_{it} = \beta_0 + \beta_1\chi_i + \beta_2\tau_t + \beta_{it}\chi_i\tau_t + x_{it}\gamma + f_i + u_{it} \quad (1)$$

$$\beta_{it} = \beta_3 + \beta_4z_{it}, \quad (2)$$

where  $\chi_i$  is a binary variable taking on the value 1 if county  $i$  is in Texas (or if  $i$  corresponds to Texas in the state data) and 0 otherwise,  $\tau_t$  is a binary variable with value 1 if time  $t$  comes on or after the effective date of the Texas constitutional amendment ( $t \geq T_0$ , where  $T_0$  is the selected treatment date) and 0 otherwise ( $t < T_0$ ),  $x_{it}$  is a  $1 \times k$  vector of exogenous conditioning variables (so the parameter vector  $\gamma$  is of dimension  $k$ ), and  $f_i$  is a mean-zero, unobserved individual fixed effect.  $\chi_i$  therefore distinguishes the ‘treatment group’ (Texas) from the ‘control group’ (all other states) while  $\tau$  distinguishes the treatment period from the non-treatment period.

Equation (2) defines the difference-in-difference estimator, our means of making inference about the effects of the amendment on spending. To interpret  $\beta_{it}$ , we can write the following conditional means implied by the model:

$$\mu_{it}^{00} = E(c_{it}|\chi_i = 0, \tau_t = 0, z_{it}, x_{it}) = \beta_0 + \gamma x_{it} \quad (3)$$

$$\mu_{it}^{10} = E(c_{it}|\chi_i = 1, \tau_t = 0, z_{it}, x_{it}) = \beta_0 + \beta_1 + \gamma x_{it} \quad (4)$$

$$\mu_{it}^{01} = E(c_{it}|\chi_i = 0, \tau_t = 1, z_{it}, x_{it}) = \beta_0 + \beta_2 + \gamma x_{it} \quad (5)$$

$$\mu_{it}^{11} = E(c_{it}|\chi_i = 1, \tau_t = 1, z_{it}, x_{it}) = \beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_4z_{it} + \gamma x_{it}. \quad (6)$$

For example,  $\mu_{it}^{11}$  in equation (6) expresses expected retail spending in Texas or one of its counties after the amendment, conditional on  $z$  and  $x$ . It is straightforward to show that

$$(\mu_{it}^{11} - \mu_{it}^{10}) - (\mu_{it}^{01} - \mu_{it}^{00}) = \beta_3 + \beta_4z_{it}, \quad (7)$$

so that  $\beta_{it}$  is the difference between the average change in spending in Texas and its counties before and after the treatment date and the average change in spending in all other counties and states before and after the same date. This difference-in-difference estimator, now a common means for program evaluation and policy interventions, controls for systematic cross-sectional and time-series effects that are unrelated to the treatment (Wooldridge 2002, p. 130). In some of our model specifications, we allow this treatment effect measure to vary linearly with the vector of exogenous variables contained in  $z_{it}$ . In most studies that use similar models,  $\beta_4$  is set to 0, so the difference-in-difference estimator is constant across  $i$  and  $t$ .

We estimate the model after combining equations (1) and (2):

$$c_{it} = \beta_0 + \beta_1\chi_i + \beta_2\tau_t + \beta_3\chi_i\tau_t + \beta_4\chi_i\tau_t z_{it} + \gamma x_{it} + f_i + u_{it}. \quad (8)$$

The coefficients describing the difference-in-difference estimator are directly obtained from the coefficients on the binary interaction term ( $\chi_i\tau_t$ ) and the three-way interaction term involving the two binary variables and  $z$ . OLS is acceptable as an estimation strategy as long as the error in (8) and the fixed effect are orthogonal to the policy action – in this case, the passage of the constitutional amendment. Standard errors used to report tests of statistical significance below are robust to heteroskedasticity and autocorrelation, and are based on the non-parametric block bootstrap procedure suggested by Bertrand *et al.* (2004, p. 265-67).

If individual fixed effects are not orthogonal to the treatment or the other right-hand-side variables, OLS estimates of (8) will be biased. Also, if there is substantial persistence in  $u$ , then coefficient estimates may be spurious. To deal with these issues, assuming the extreme case that  $u$  is a random walk ( $u_{it} = u_{it-1} + \epsilon_{it}$ , where  $\epsilon$  is white noise), we first-difference the

time-varying variables, for each cross-sectional unit,  $i$ :

$$c_{it} - c_{it-1} = \beta_2(\tau_t - \tau_{t-1}) + \beta_3\chi_i(\tau_t - \tau_{t-1}) + \beta_4\chi_i(\tau_t z_{it} - \tau_{t-1} z_{it-1}) + (x_{it} - x_{it-1})\gamma + \epsilon_{it}. \quad (9)$$

First-differencing eliminates both the unobserved fixed effect and the unit root in the error term, which can possibly lead to improved estimates. For  $T > 2$ , the treatment time dummy  $\tau_t$  becomes an ‘intervention’ variable, taking on a unit value only at the period of the treatment, with zeroes before and after.

## 3.2 Data

We rely on data from three sources, one for retail sales and income at the county-level and two at the state-level. In all cases, we restrict our sample period to be 1992 to 2002. We do so to avoid the years of high oil-price volatility of the 1980’s, which makes it more likely that the Texas economy followed similar trends to other states, and the Tax Reform Act of 1986, which altered the relative price of home-equity lending (by eliminating tax-deductability of interest on all credit except mortgage debt) for households in all states except Texas. The ending period of our sample also avoids the 2003 changes in Texas to allow home-equity lines of credit and the housing price bubble of the early to mid 2000’s.

We obtain retail sales by county from the Economic Census, Retail Trade Series. These data are sales for establishments with payroll, and are available only at 5-year intervals, so we use observations from the 1992, 1997 and 2002 censuses. Of the 3143 counties in the US during this period and 254 in Texas, we use the 3006 and 244 counties, respectively, that have complete records for both sales and income for these years. Thus, there are 9018 observations in our balanced panel.<sup>10</sup>

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<sup>10</sup>The raw data for retail sales and the other county level series are available from the US Census Bureau,. Spreadsheets can be downloaded from CenStats: <http://www.census.gov/support/DataDownload.htm> The same data in report form are also accessible: [www.census.gov/prod/1/bus/retail/92area/92ret.html](http://www.census.gov/prod/1/bus/retail/92area/92ret.html), [www.census.gov/prod/www/abs/ec1997retail.html](http://www.census.gov/prod/www/abs/ec1997retail.html), and [www.census.gov/prod/www/abs/ec2002retail.html](http://www.census.gov/prod/www/abs/ec2002retail.html)

The Census defines retail sales as “merchandise sold for cash or credit at retail and wholesale by establishments primarily engaged in retail trade; amounts received from customers for layaway purchases; receipts from rental of vehicles, equipment, instruments, tools, etc.; receipts for delivery, installation, maintenance, repair, alteration, storage, and other services; the total value of service contracts; and gasoline, liquor, tobacco, and other excise taxes which are paid by the manufacturer or wholesaler and passed on to the retailer.” Retail sales are net of deductions for refunds and allowances for goods returned by customers, and do not include taxes collected from customers and forwarded to taxing authorities, gross sales and receipts of departments or concessions operated by other companies, and receipts from the sale of government lottery tickets.

Beginning in 1997, the North American Industry Classification System (NAICS) replaced the Standard Industrial Classification (SIC) system as the official means for classifying business establishments. This change may affect the integrity of the county-level data, because in part it redefines extant industries, identifies new industries, and alters how they relate to retail sales.<sup>11</sup> Table 1 shows state-wide data comparing sales in 1997 under the alternative classification systems (for all but three states), the only year the Census Bureau reports both. Whereas on average across states sales in 1997 under the NAICS were only 2.5% less than under the SIC, they were only 0.4% less in Texas; thus, our estimates of the treatment effect could be biased upwards.<sup>12</sup> To mitigate this potential bias, we convert each year’s data to an SIC basis by multiplying the NAICS sales data in 1997 and 2002 for each county by its corresponding state’s ratio of 1997 SIC-to-NAICS sales.<sup>13</sup>

County-level data on personal income are from the Census Bureau (CenStats), as orig-

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<sup>11</sup>Under the NAICS, eating and drinking establishments, mobile food services, pawn shops and bakeries have been excluded from retail sales, while sales from automotive supply dealers, computer and peripheral equipment merchants, and office supply dealers have been added. For additional details, see <http://www.census.gov/eos/www/naics/>.

<sup>12</sup>But note that sales *rose* in five states. The relatively small states of Hawaii and South Dakota had the biggest changes.

<sup>13</sup>For example, for each county in Alabama, we multiply observed sales in 1997 and 2002 by the factor 1.0271. The underlying assumption here is that there is no significant variation across counties within a state regarding the SIC/NAICS ratio, and that the 1997 ratio is reasonably constant over time.

inally compiled by the Bureau of Economic Analysis. Data on county-level personal taxes are not available, so we do not measure disposable income. We deflate all nominal values by the overall consumer price index (all urban consumers), obtained from the Bureau of Labor Statistics, and reduce to per capita values using population measures obtained from the Current Population Survey. County-level median housing price data used in some of the models below come from the Census Bureau as well, at the link above.

We could aggregate the county-level sales data to the state-level, but would be limited to the five-year temporal frequency. However, we have found two sources of annual state-wide retail sales. The first is the retail sales data imputed from quarterly state sales tax revenues and tax rates by Garrett, Hernández-Murillo, and Owyang (2005). Dividing the former by the latter yields estimates of overall sales (at least those subject to sales taxes) by state. We have obtained their data, but have also recomputed and verified the imputed sales series from the original sources: for revenues, the *State Government Collections* data base from the Census Bureau (general sales and gross receipts, item T09) and for tax rates the Tax Foundation’s *Facts and Figures on Government Finances*. The sample is limited to 43 states, because five states (Alaska, Delaware, Montana, Oregon, and New Hampshire) have no sales tax and two states (Nevada and Utah) have incomplete tax revenue records. Importantly, sales by this measure are in no way susceptible to bias from the 1997 changes in industry classification. We construct annual data from 1992 to 2002 by summing the original quarterly data yearly, for a balanced panel of 473 observations.

Our second set of annual retail sales data at the state level comes from the *Statistical Abstract of the United States*.<sup>14</sup> The Census Bureau obtains these sales figures from *The Survey of Buying Power Data Service: Market Statistics*. The *Abstract* contains retail sales by state for all years in our sample except (inexplicably) for 1999. As an initial approach to filling in these missing data, we have imputed 1999 values for each state by using the ratio

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<sup>14</sup>[http://www.census.gov/compendia/statab/past\\_years.html](http://www.census.gov/compendia/statab/past_years.html), editions 2004 – 05, 2003, 2001, 2000, 1999, 1998, 1997, 1996, table “Retail Sales by Type of Store and State,” series “Total sales, all stores.”

of retail sales to shopping center sales in 2000, and applying this ratio to shopping center sales by state for 1999 (which *are* available). We use these series as a robustness check on our other data. However, because of the imputation of 1999, and because these data are also potentially sensitive to the NAICS re-classification, we recommend caution in interpreting the results.<sup>15</sup>

For the state-wide data, we use state personal income net of personal taxes as our measure for state disposable income. These nominal magnitudes are deflated and converted to per capita values using the same price index and population estimates noted above.

Figure 1 plots the three retail sales series over time, by state. We have aggregated the (reclassification adjusted) county data to the state-level for 1992, 1997 and 2002. A quick glance suggests that, while longer-run trends are generally similar, the tax-based series and the Census-based series provide different information about fluctuations in sales over the period 1992 to 2002. There is a general tendency for the county-aggregates to conform closely to the annual census data in 1992 and 1997, but to deviate in 2002. There is some indication that the reclassification of industries affects the annual census data (in blue) since it is unadjusted, with some state sales rising noticeably, but others falling after the change. However, keep in mind that the adjustment we made at the county-level for almost every state *increases* both the 1997 and 2002 county series.

Despite the caution attached to the annual census data for the states, examining the three Texas series in the figure is instructive. It is the only state for which sales clearly rise by a large magnitude in 1999 for *both* annual series, which is suggestive but not conclusive of the existence of a treatment effect. At the same time, there is no apparent rise in the aggregate county-wide data for this state from 1997 to 2002. The results from the difference-in-difference model, reported in the next section, can help sort out the potential treatment effects.

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<sup>15</sup>We are currently working to adjust these series to allow for the break in classification.

## 4 Results

### 4.1 County-level

Panel A of Table 2 contains estimates of the parameters in equation (8) – the specification in levels – for the county-level data. We set  $t = 1992, 1997$  and  $2002$  in all county-wide regressions, so there are 9018 observations ( $n = 3006$  counties,  $T = 3$  time periods). Since the changes in home-equity laws took effect in 1998, with subsequent changes in 1999, we consider the time period 1992 and 1997 to be pre-treatment, and 2002 to be post-treatment, and set  $\tau$  accordingly.

We first estimate the model with the restrictions that  $\beta_4 = \gamma = 0$ . In effect, this specification gives sample moments of the unconditional means of the cross-county and cross-period subsets defined by the binary variables. The coefficient estimates, reported in column 1 of the panel, imply that the unconditional mean of log real per capita sales in non-Texas counties prior to the amendment is 8.236, the unconditional mean in Texas during this period is 8.134, the unconditional mean outside of Texas post-treatment is 8.329, and the unconditional mean in Texas post-treatment is 8.2124 (as implied by equations 3 through 6). Although the coefficient on  $\beta_3$  suggests that the change in spending in Texas counties pre- to post-amendment was 1.4% *less* than the corresponding change in non-Texas counties, and is thus not consistent with the credit-constraint hypothesis, the magnitude is small and its robust t-statistic suggests that the difference-in-difference effect, conditional on this simple specification with no controls, is not statistically different from zero.

Column 2 reports results for the model in which we maintain the restriction that  $\beta_4$  be zero, but include in  $x_{it}$  a complete set of state dummies, a time trend, and an interaction term between each state dummy and the time trend to allow for state-specific trends. The regression corresponding to column 3 adds the log of real per capita income ( $y_{it}$ ) to  $x_{it}$ . Here, the estimates of  $\beta_3$  show positive treatment effects, and are therefore consistent with the existence of credit constraints: the change in Texas retail spending exceeds the change

in non-Texas retail spending across the treatment dates by almost 5% without income as a control and 2.3% when income is included. The former estimate is statistically different from zero at a 5% level, while the latter is significant at 10%. The magnitude of these effects exceeds the difference-in-difference estimates of Leth-Petersen, which were at most 1% for the Danish ‘experiment.’<sup>16</sup>

Because these results are for the *average* treatment effect of reducing borrowing constraints in Texas, they can mask variation in the effect across counties. To examine this possibility, we consider two additional specifications that relax the restriction that  $\beta_4 = 0$ . In the first, we set  $z_{it} = y_{it}$  (log real per capita income); in the second, we allow for non-linearity by letting  $z_{it}$  be a time-invariant binary variable that distinguishes low-income from high-income counties prior to the amendment. Specifically, we set  $z_i$  to 1 if county  $i$ ’s county-wide real per capita income in 1992 is less than the median real per capita income in Texas in 1992, and zero otherwise.<sup>17</sup> Our rationale for linking the treatment effect to income is that, on average, households in low-income counties are likely to hold less of their wealth in liquid assets than households in high-income counties, and would therefore more likely be liquidity-constrained.<sup>18</sup>

Column 4 reports estimates of the model when  $z$  is set to income. If our hypothesis above regarding income, liquid assets and liquidity-constraints is valid, we expect  $\beta_3$  to be positive and  $\beta_4$  to be negative. Our estimates support this hypothesis, and are statistically significant. To give a feel for magnitudes, Panel B of the table shows the predicted treatment effects for various income levels. The table gives, in the the lower part, the fitted values of equation (2) using the estimates in column 4 of Table 2, panel A, and the selected percentiles of real per capita income shown in the upper part of the panel (using logs). For the median

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<sup>16</sup>Our estimate of the income elasticity of per capita sales is approximately one, which can be interpreted in at least two ways. First, cross-sectional variation in income may be a good proxy for cross-county variation in wealth, so that the coefficient of unity is plausible in a permanent-income sense. Second, there might be a spurious regression problem if the error term has a unit root.

<sup>17</sup>Our results below do not change importantly when we recompute the dummy for 1997 instead of 1992.

<sup>18</sup>Such an assumption for individuals, recall, is the basis on which Leth-Petersen identifies the treatment effect in his study.

Texas county (averaged over the entire period 1992 to 2002), growth in spending around the passage of the amendment was just over 4% higher than growth in spending outside of Texas, about double the average effect. For lower-income counties at the 25<sup>th</sup> and 10<sup>th</sup> percentiles, the treatment effect is 3 and a half to almost 6 times the average effect. Higher-income Texas counties at the 75<sup>th</sup> percentile had changes in spending that were no different than non-Texas counties, while those at the 90<sup>th</sup> percentile decreased spending by 3.8% relative to the control group.

The latter implication is counter-intuitive: if higher-income, highly liquid households or communities are not credit-constrained, then we would expect their spending to be no different from the control group, not less, all things the same. Our alternative interaction model allows the marginal effects of the treatment to differ across the income distribution. In this case,  $\beta_3$  is the difference-in-difference estimate for the upper-income counties while the sum  $\beta_3 + \beta_4$  is the estimate for low-income counties. The effect of the low-income group is 7.3% and that of the high-income group is  $-0.028$ , although the latter may differ from zero because of sampling error. The puzzle of decreasing high-income county spending remains, but it is of a smaller magnitude than for the linear specification.

Another implication of the credit-constraint hypothesis is that counties in Texas with the greatest house-price appreciation during this period would have the greatest potential for using home-equity loans; for current homeowners, an increase in home value is dollar-for-dollar an increase in equity. To examine this possibility, we set  $z_{it}$  to be 1 for counties in which the change in real housing value from 1990 to 2000 was in the top 50% of the Texas cross-county price-appreciation distribution, and zero otherwise. The estimates in column 6 imply that the effect for high-appreciation areas was 6.4% while being essentially zero for low-appreciation areas. This finding is in line with the the credit-constraint story, although we might expect a positive effect for the latter group, albeit smaller than the former group.

We also estimate the last five specifications above, but in the first-differenced form of

equation (9).<sup>19</sup> Table 3 reports these results. For the specifications in which  $\beta_4 = 0$  and the treatment effect is constant (column 1 for  $\gamma = 0$  and column 2 for  $\gamma$  unconstrained), the estimates imply almost a 5% treatment effect (just over 4% with the income control), around twice the effect measured in Table 2. In column 3, when  $z_{it} = y_{it}$  the statistically significant estimates of  $\beta_3 = 1.16$  and  $\beta_4 = -0.12$  imply a treatment effect of 6% for the 25<sup>th</sup> percentile income level and 8% at the 10<sup>th</sup> percentile, somewhat smaller than for the levels model. For the 90<sup>th</sup> percentile, the effect is now 2% (and thus no longer negative). This result is roughly consistent with the non-linear specification in column 4, where the low income counties exhibit a treatment effect of almost 6% while the high income counties maintain a positive but smaller response. In the final column, we see that counties in the upper-house-price-appreciation group have an effect 1.5% larger than the low-appreciation group, which now shows a positive effect, again eliminating the minor puzzle from the levels specification.

## 4.2 State-level

We now consider the effects of the Texas amendment on retail spending at the state level. One important advantage of the state data is that time-variation is richer than for the county-level data. In particular, we observe spending on an annual basis for the states, allowing us to get a clearer picture of the timing of the amendment's effects on spending. The 5-year periods of the county-level data are limiting in this regard. As we've noted, in light of the secondary clarifying amendments in 1999, the full impact of the increase in credit availability may not have been immediately felt, so dynamic effects might be important. One drawback of the state data is that the cross-sectional variation at such an aggregated level is less informative, relative to the county data, about differences in the treatment effect due

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<sup>19</sup>As noted above,  $\beta_0$  and  $\beta_1$  are not identified in this model. Because of differencing, we lose 3006 cross-county observations. State dummies remain in the specification even after differencing because of the state-trend interaction terms in the levels specification.

to wealth, liquid assets and house price appreciation. Therefore, all specifications in this section impose the restriction that  $\beta_4 = 0$ , the difference-in-difference estimates are constant over time and states.

We first discuss results when we use the sales data implied by tax revenues (Table 4) and then discuss results for the Census survey data (Table 5). For each case, the data are annual and range from 1992 to 2002 as for the county-level analysis. We set  $T_0$  to be 1999, so that  $\tau$  takes on unit values beginning in that year. As with the county-level data, the dependent variable in the regressions is the log of real per capita sales (or its first-difference), now measured at the state-level.

Panel A of Table 4 gives the results for our first measure of retail sales based on tax revenues, using the levels model of equation (8). We consider three specifications, presented in the three columns of the table, respectively: 1) no control variables (to obtain unconditional means), 2) log real per capita income only in  $x_{it}$ ; and 3) income, state dummies and a time trend as controls.<sup>20</sup>

On average, real per capita retail spending rose in Texas by 17% from the pre-treatment period, 1991 to 1998, to the post-treatment period, 1999 to 2002 ( $\beta_2 + \beta_3$  in column 1). Before the amendment, spending in Texas was 23% less than spending in other states ( $\beta_1$ ), while spending increased by more than 7% in non-Texas states from the pre-treatment period to the post-treatment period ( $\beta_2$ ). Regarding the treatment effect of the amendment,  $\beta_3$  implies that real per capita spending in Texas increased by more than 10% compared to the change in non-Texas spending. As seen in columns 2 and 3,  $\beta_3$  remains robust to the addition of real per capita income, state dummies and a time trend – with income only in  $x$  the estimate is 9.6%, with the fixed effects terms as well the estimate drops by 100 basis points.

Panel B shows the results for the first-differenced model. Here, the results are more robust across the three models, with  $\beta_3$  just over 15% for each case, although the effect rises

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<sup>20</sup>As for the county-level data, we ran a specification including state-trend interaction terms. Adding these terms tended to reduce the model’s explanatory power in terms of  $\bar{R}^2$ , so we do not put much weight on the results and therefore do not report them. The  $\beta_3$  estimates were actually larger for these models.

by about two-thirds over the corresponding levels model. Overall, there is strong evidence for the importance of credit-constraints at the state level. The magnitude of the effects tends to be larger than for the county-level estimates, which likely reflects strong responses to spending before 2002 that cannot be observed in the county data.

Does this evidence hold for our alternative measure of state retail sales based on survey data? Panels A and B of Table 5 show that it generally does for both the levels and first-difference specifications. The magnitude of the treatment effects here falls in the low end of the range found for the tax-revenue sample, but also shows less variation across model specification. Unconditionally, the difference-in-difference estimate is 8.2% (Panel A, column 1). It falls to just under 6% when income, state dummies and a time-trend are added (panel A, column 3). The estimates from the first-differenced model reported in panel B are in the neighborhood of 8.5%, with remarkably little variation across specifications.

Finally, in Table 6 we report estimates and t-statistics for  $\beta_3$  when we vary  $T_0$ , the assumed effective date of the natural experiment. As we've noted, the annual frequency of the state-wide data allows us to better estimate the timing of potential effects. To this end, we alternatively set  $T_0=1998, 2000, \text{ and } 2001$ , and re-estimate the models for both our tax-revenue-based sales and census-based sales data. (In the table, we repeat the baseline results for  $T_0 = 1999$  given in previous tables, for ease of comparison.) For the tax-revenue data, the treatment effects are largest for the original  $T_0 = 1999$  dummy, across models and specifications. Whereas  $\beta_3$  rises for the first-differenced model in this case, it becomes negligible for 1998, 2000 and 2001 cases. This same pattern holds for the Census data, with the exception that for specification 3 the effects estimated from equations (8) and (9) are essentially the same. 1999 clearly stands out as the year in which the treatment effects was most important.

### 4.3 Discussion

Overall, we find that the Texas home-equity amendment had substantial effects on retail spending in Texas at both the county and state levels. At the county-level, our difference-in-difference estimates of the treatment effect of this ‘natural experiment’ suggest around a 4% to 5% effect on spending in the average Texas county, with larger effects for lower-income counties and counties experiencing higher house-price-appreciation during the 1990’s. We find stronger effects for the state data – ranging from 5% to 15%, over both data sets and model specifications – most likely reflecting the finding that the biggest effect occurs in 1999, a year in which spending is not observed in the county data. The findings are consistent with the idea that the amendment significantly relaxed binding borrowing constraints on credit-constrained Texas consumers.

How reasonable are the magnitudes of the effects we estimate? Consider an average treatment effect at the county-level of 4%. Such an effect implies that real per capita sales in Texas were 4% higher than had there been no change in the law, all else the same. Given that state-wide population grew by about 1.5% from 1997 to 1998 and inflation was in the range of 2%, this estimate implies that retail sales increased 7.5% because of the law. Nominal retail sales in Texas in 1997 totaled roughly \$176 billion; so our estimates imply an increase of \$13.2 billion. Using the lower bound estimates of home-equity lending of \$20 billion noted above, our results suggest that more than 50% of new home-equity loans were used, ultimately, for retail spending in Texas.

While this magnitude seems large, it is not unreasonable. First, total home-equity lending most likely exceeded the lower-bound estimates above. Second, the effects of relaxing credit-constraints need not work solely and directly through current home-equity loans. Household perceptions that constraints will be less likely to bind in the future can lead households to reduce precautionary saving and increase current spending, even without incurring home-equity debt, a point emphasized by Gross and Souleles (2002). They find that an individual’s

marginal propensity to consume out of liquid assets ranges from 10 to 14%, on average. While their estimated effect is much larger for those clearly credit-constrained, its is also large and significant for those not currently constrained (in their case, credit-card holders far from the borrowing limit), which is consistent with a reduction in precautionary saving. Iacoviello (2004) estimates a long-run aggregate consumption response to house prices of 15%, which gives some credence and support to the magnitudes of our findings for the state level data.<sup>21</sup> Finally, Duca, Gould and Taylor (1998, p. 9) cite a University of Texas study that “estimated that the temporary stimulus [of the home-equity act] could be equivalent to a 9-percent increase in Texas retail sales.”<sup>22</sup> As noted, and for reasons we can’t explain, our estimated effects are much larger than those reported by Leth-Petersen (2010). An interesting issue for future work is to consider why two similar experiments had such different effects.

We’ve mentioned above that industry classifications were altered beginning in 1998. The timing of this change is not ideal for our study given its coincidence with the Texas amendment. However, this change will bias our estimates only to the extent that the changes had unique effects on Texas relative to the other states; common effects will be factored out in the difference-in-difference model. (For example, if the relative contribution of each industry to total sales in each of the 50 states is identical, the re-classification would not bias our results.) In addition, we have adjusted our county-level data to control for the change, and the state-wide tax-revenue sales data are completely immune to the reclassification, so the state-wide Census data alone are potentially susceptible. Yet the  $\beta_3$  estimates for these data are *smaller* than those for the tax-revenue based data, so it seems unlikely that re-classification biases upwards our main findings.

Finally, we see our results as providing further evidence that the link between house prices and non-housing expenditures is most likely the result of ‘collateral effects’ rather than ‘wealth effects.’ From the point of view of wealth, the Texas experiment did nothing directly

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<sup>21</sup>All else the same, a dollar increase in housing value is a dollar increase in home-equity.

<sup>22</sup>We have not been able to locate the original study.

for homeowners. But for credit-constrained homeowners, the home-equity amendment was roughly tantamount to having their housing stock rise in value from their current debt liability to (80% of) the market value of the home.<sup>23</sup> Spending responses to this natural experiment are thus most likely caused by the newly available collateral, not newly perceived wealth.

## 5 Conclusion

The findings of this paper suggest that credit constraints matter, at least for retail spending in Texas. While we can't necessarily generalize to other states, our difference-in-difference model estimates of the retail sales effect of the Texas natural experiment provides further evidence for the importance of credit-constraints generally, and the relevance of collateral constraints as a means for housing prices to affect overall economic behavior. In future work, we plan on using individual data to study this event, allowing us to more closely compare our results to those of Danish housing-equity changes and perhaps understand better the economic mechanisms at work.

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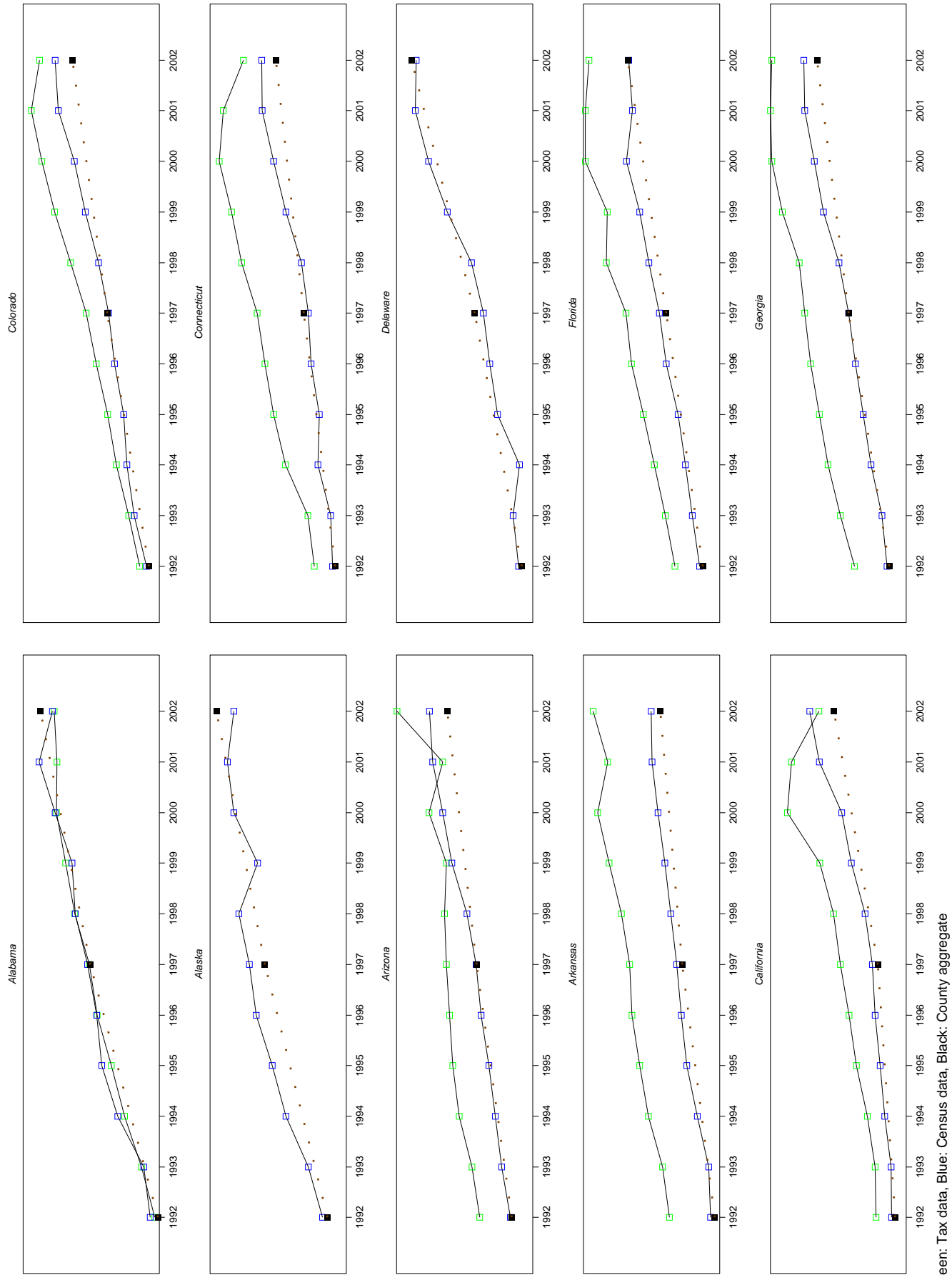
<sup>23</sup>Unlike most other studies, Leth-Petersen (2010) excluded, we thus estimate the effect of a big, discrete exogenous shock, rather than an incremental or marginal one. This is potentially another reason why we find seemingly large spending effects.

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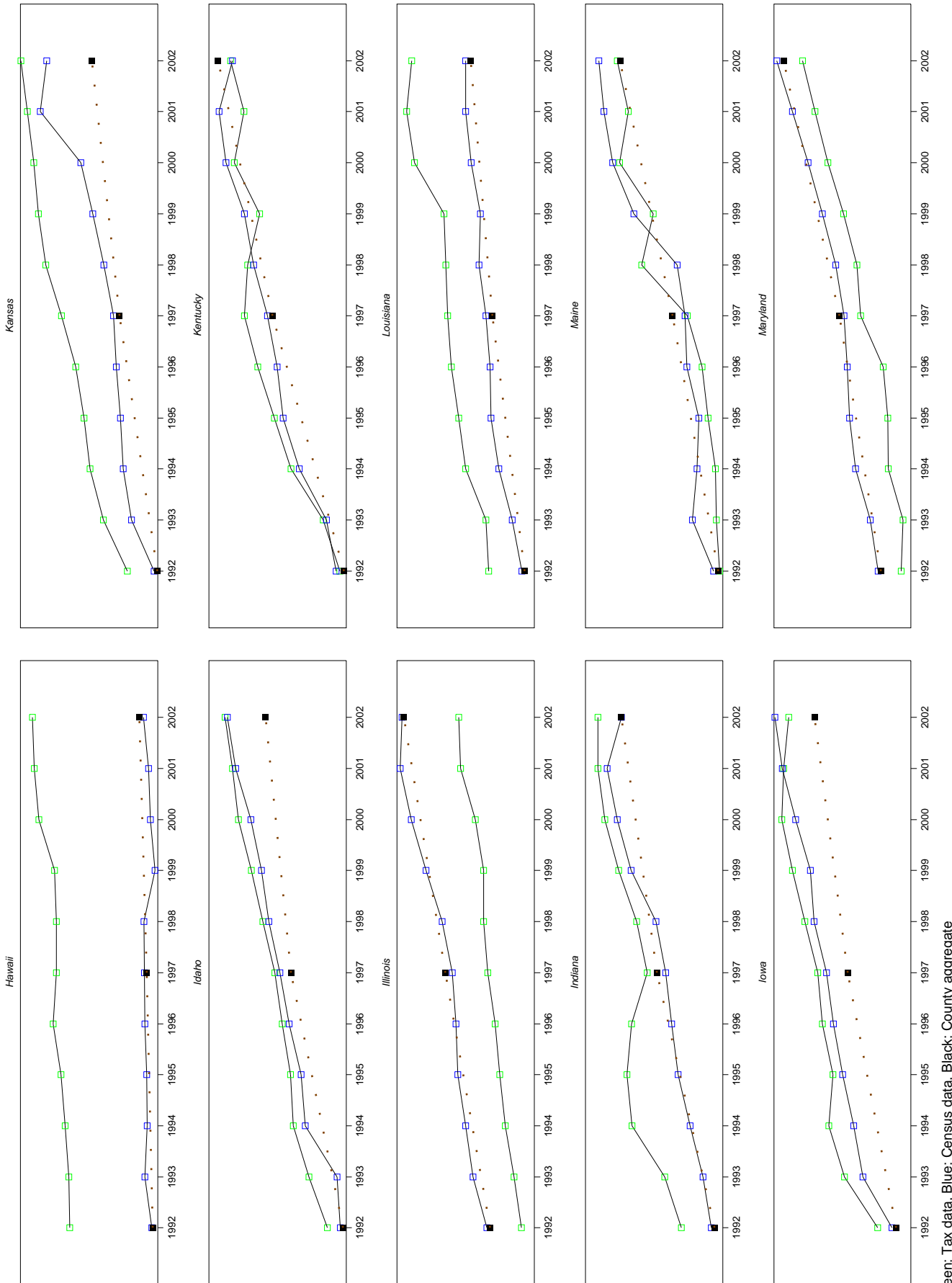
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Figure 1. Retail sales data



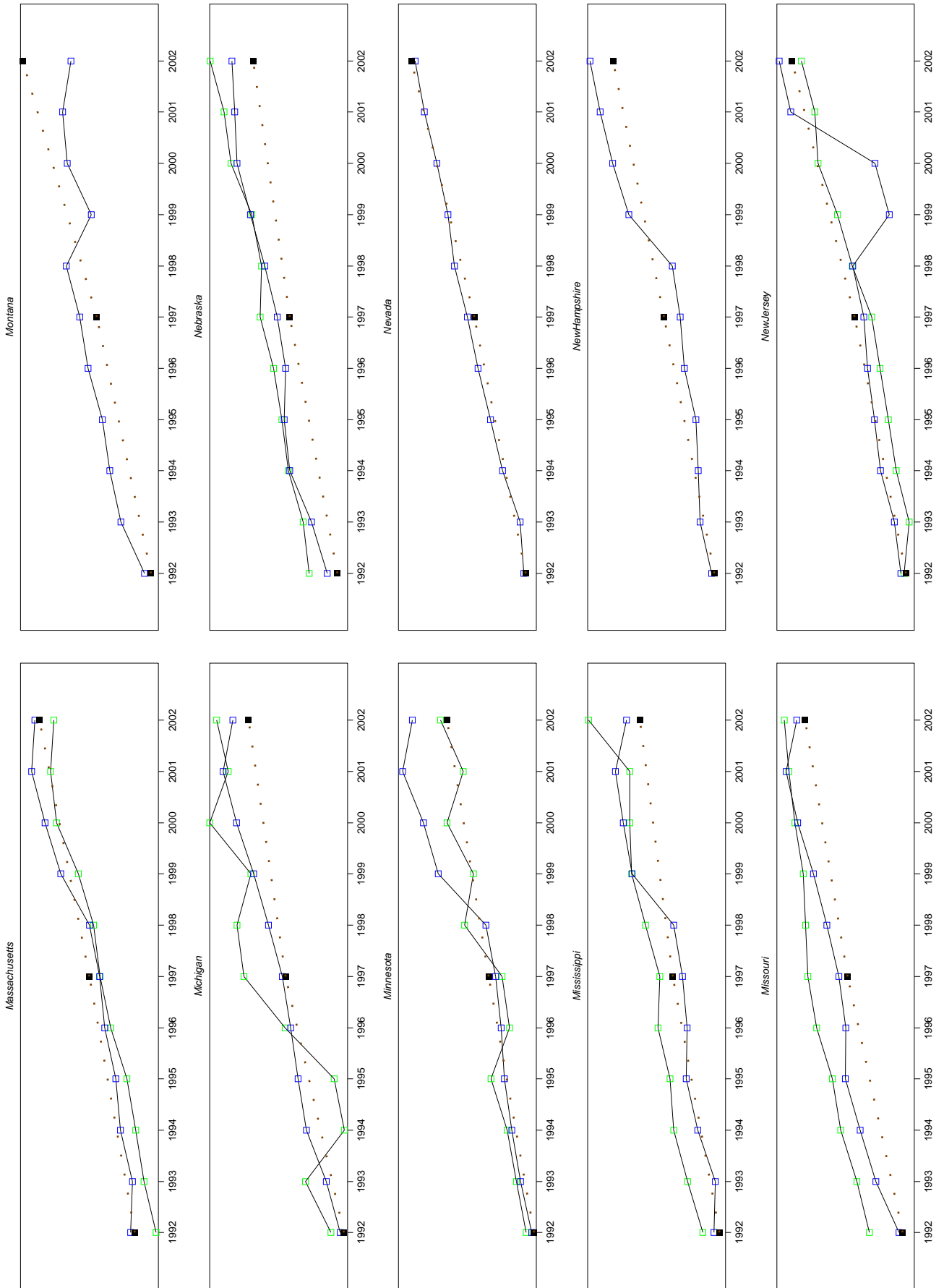
Green: Tax data, Blue: Census data, Black: County aggregate

Figure 1 (cont). Retail sales data



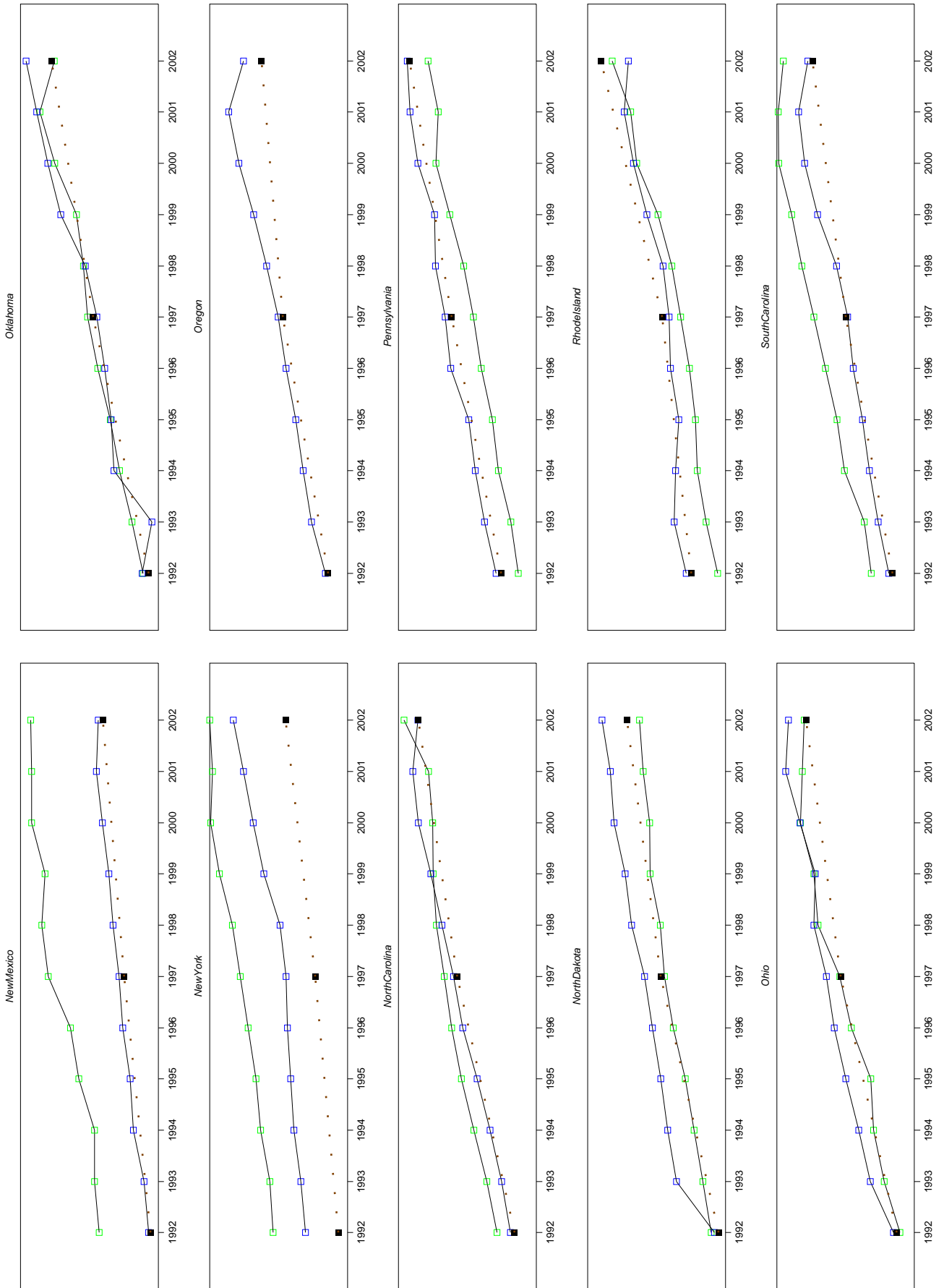
Green: Tax data, Blue: County aggregate

Figure 1 (cont). Retail sales data



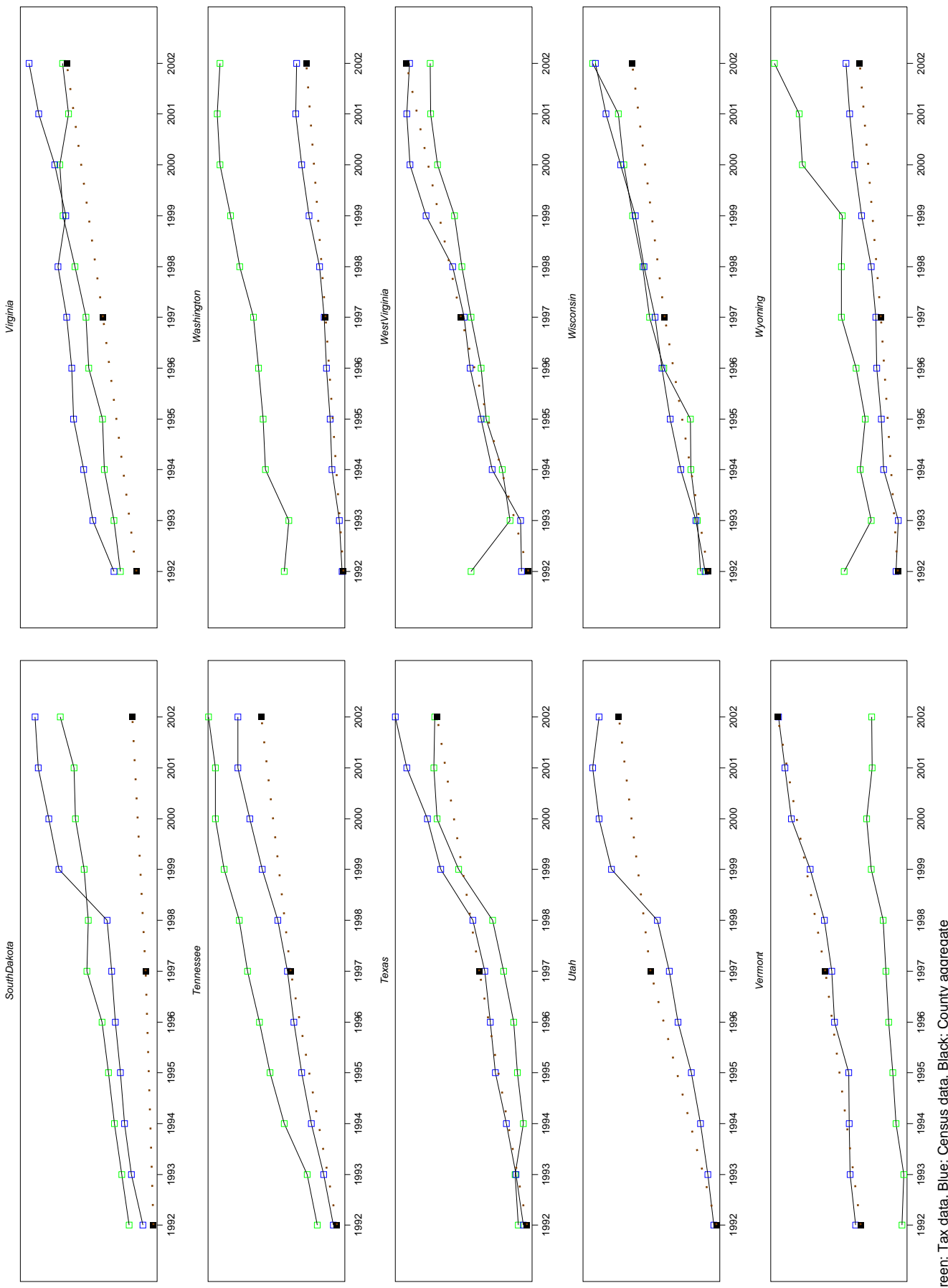
Green: Tax data, Blue: County aggregate

Figure 1 (cont). Retail sales data



Green: Tax data, Blue: County aggregate, Black: Census aggregate

Figure 1 (cont). Retail sales data



Green: Tax data, Blue: County aggregate, Black: County aggregate

Table 1: SIC and NAICS Retail sales, 1997

		SIC	NAICS	% change
1	Alabama	37,614,376	36,623,327	-0.0263
2	Alaska	6,598,019	6,251,372	-0.0525
3	Arizona	44,916,454	43,960,933	-0.0213
4	Arkansas	21,903,527	21,643,695	-0.0119
5	California	<i>NA</i>	263,118,346	<i>NA</i>
6	Colorado	42,188,444	40,536,034	-0.0392
7	Connecticut	35,832,852	34,938,893	-0.0249
8	Delaware	8,555,393	8,236,970	-0.0372
9	Florida	158,693,907	151,191,241	-0.0473
10	Georgia	74,096,020	72,212,484	-0.0254
11	Hawaii	12,837,212	11,317,752	-0.1184
12	Idaho	11,510,569	11,649,609	0.0121
13	Illinois	111,805,925	108,002,177	-0.0340
14	Indiana	59,181,921	57,241,650	-0.0328
15	Iowa	26,283,470	26,723,822	0.0168
16	Kansas	23,080,403	22,571,918	-0.0220
17	Kentucky	34,364,705	33,332,675	-0.0300
18	Louisiana	37,222,865	35,807,894	-0.0380
19	Maine	13,275,829	12,737,087	-0.0406
20	Maryland	48,297,830	46,428,206	-0.0387
21	Massachusetts	62,533,487	58,578,048	-0.0633
22	Michigan	96,836,422	93,706,078	-0.0323
23	Minnesota	48,814,277	48,097,982	-0.0147
24	Mississippi	20,923,871	20,774,508	-0.0071
25	Missouri	52,001,235	51,269,881	-0.0141
26	Montana	8,014,759	7,779,112	-0.0294
27	Nebraska	16,350,932	16,529,333	0.0109
28	Nevada	19,019,702	18,220,790	-0.0420
29	New Hampshire	16,264,348	15,812,027	-0.0278
30	New Jersey	81,672,814	79,914,892	-0.0215
31	New Mexico	15,585,757	14,984,454	-0.0386
32	New York	148,865,467	139,303,944	-0.0642
33	North Carolina	74,507,525	72,356,763	-0.0289
34	North Dakota	6,382,015	6,702,134	0.0502
35	Ohio	107,417,375	102,938,830	-0.0417
36	Oklahoma	28,306,597	27,065,555	-0.0438
37	Oregon	34,066,578	33,396,849	-0.0197
38	Pennsylvania	113,092,636	109,948,462	-0.0278
39	Rhode Island	8,207,824	7,505,754	-0.0855
40	South Carolina	34,912,588	33,634,264	-0.0366
41	South Dakota	9,872,544	11,707,133	0.1858
42	Tennessee	52,750,245	50,813,221	-0.0367
43	Texas	183,274,112	182,516,112	-0.0041
44	Utah	20,110,336	19,964,601	-0.0072
45	Vermont	6,018,347	5,898,646	-0.0199
46	Virginia	64,575,911	62,569,924	-0.0311
47	Washington	<i>NA</i>	52,472,866	<i>NA</i>
48	West Virginia	14,639,608	14,057,933	-0.0397
49	Wisconsin	51,066,574	50,520,463	-0.0107
50	Wyoming	<i>NA</i>	4,530,537	<i>NA</i>
	<i>Mean</i>			-0.0245

Notes: Sales figures (thousands of current dollars) for retail establishments with payroll. Data source is the 1997 Economic Census (Comparative Statistics). The final column shows the percentage change from SIC to NAICS.

Table 2: County-wide data, model in levels

Panel A: Estimation results						
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_0$	8.2357 ( 336.89 )	7.9902 ( 27.64 )	-2.6413 ( -2.44 )	-2.7607 ( -2.56 )	-2.695 ( -2.5 )	-2.6316 ( -2.42 )
$\beta_1$	-0.1023 ( -1.9 )	0.0271 ( 0.11 )	-0.0376 ( -0.28 )	-0.0383 ( -0.29 )	-0.0379 ( -0.29 )	-0.0375 ( -0.28 )
$\beta_2$	0.0929 ( 11.92 )	-0.0868 ( -7.77 )	-0.075 ( -7.1 )	-0.0749 ( -7.07 )	-0.0749 ( -7.09 )	-0.075 ( -7.1 )
$\beta_3$	-0.0139 ( -1.52 )	0.0496 ( 1.99 )	0.0227 ( 1.67 )	3.4454 ( 1.96 )	-0.0278 ( -1.55 )	-0.0095 ( -0.91 )
$\beta_4$				-0.3633 ( -1.96 )	0.1007 ( 1.99 )	0.0639 ( 1.99 )
$\gamma$			1.1541 ( 10.15 )	1.167 ( 10.29 )	1.1599 ( 10.23 )	1.153 ( 10.1 )
$\bar{R}^2$	0.01	0.104	0.273	0.273	0.273	0.273
$NT$	9018	9018	9018	9018	9018	9018
State dummies	NO	YES	YES	YES	YES	YES
Time trend	NO	YES	YES	YES	YES	YES
State-trend interaction	NO	YES	YES	YES	YES	YES

Panel B: Treatment effect by (log) percentile of per capita income				
	1992	1997	2002	Mean
Texas income distribution				
10 %	8863.962	8826.177	9630.703	9016.682
25 %	10138.96	10516.61	11066.3	10569.77
50 %	11113.85	11690.33	12379.87	11728.7
75 %	12396.34	12984.48	13890.69	13105.09
90 %	14082.3	14788.41	15688.71	14608.39
Implied treatment effects				
10 %	0.143	0.145	0.113	0.137
25 %	0.094	0.081	0.062	0.079
50 %	0.061	0.043	0.022	0.041
75 %	0.021	0.004	-0.02	0.001
90 %	-0.025	-0.043	-0.064	-0.038

Notes: Panel A presents estimates from equation (8):  $c_{it} = \beta_0 + \beta_1\chi_i + \beta_2\tau_t + \beta_3\chi_i\tau_t + \beta_4\chi_i\tau_t z_{it} + \gamma x_{it} + u_{it}$ ,  $t = 1992, 1997, 2002$ .  $\gamma$  is the coefficient on log per capita real income. In Panel B we use the parameter values for  $\beta_3$  and  $\beta_4$  from column 4 of Panel A.  $NT$  is the number of panel observations.  $R^2$  denotes the adjusted R squared. Heteroscedasticity- and autocorrelation-consistent  $t$ -statistics (in parenthesis) are from a non-parametric bootstrap with 1000 replications; they account for intra-state (clusters) correlation (see., Bertrand *et al.* (2004)).

Table 3: County-wide data, model in differences

	(1)	(2)	(3)	(4)	(5)
$\beta_2$	-0.0868 ( -7.77 )	-0.083 ( -7.93 )	-0.083 ( -7.93 )	-0.083 ( -7.93 )	-0.083 ( -7.93 )
$\beta_3$	0.0496 ( 1.99 )	0.0409 ( 1.95 )	1.1567 ( 2.09 )	0.0267 ( 1.79 )	0.033 ( 1.89 )
$\beta_4$			-0.1184 ( -2.08 )	0.0284 ( 2.1 )	0.0156 ( 2.06 )
$\gamma$		0.3744 ( 4.17 )	0.3742 ( 4.17 )	0.3728 ( 4.17 )	0.3731 ( 4.16 )
$\bar{R}^2$	0.058	0.072	0.072	0.072	0.071
$NT$	6012	6012	6012	6012	6012
State dummies	YES	YES	YES	YES	YES

Notes: Estimates from equation 9:  $c_{it} - c_{it-1} = \beta_2(\tau_t - \tau_{t-1}) + \beta_3\chi_i(\tau_t - \tau_{t-1}) + \beta_4\chi_i(\tau_t z_{it} - \tau_{t-1} z_{it-1}) + \gamma(x_{it} - x_{it-1}) + \epsilon_{it}$ ,  $t = 1992, 1997, 2002$ . See notes to Table 2, Panel A.

Table 4: State-wide data, tax-revenue based

	(1)	(2)	(3)
<b>Panel A: Estimation results, model in levels</b>			
$\beta_0$	8.7821 ( 202.11 )	5.6160 ( 2.06 )	0.8469 ( 0.46 )
$\beta_1$	-0.2312 ( -1.94 )	-0.2254 ( -1.95 )	-0.1006 ( -0.58 )
$\beta_2$	0.0714 ( 7.23 )	0.0364 ( 1.11 )	-0.0253 ( -2.85 )
$\beta_3$	0.1027 ( 2.02 )	0.0959 ( 2.02 )	0.0860 ( 2.00 )
$\gamma$		0.3341 ( 1.15 )	0.8242 ( 4.16 )
$\bar{R}^2$	0.023	0.044	0.953
$NT$	473	473	473
State dummies	NO	NO	YES
Time trend	NO	NO	YES
<b>Panel B: Estimation results, model in differences</b>			
$\beta_2$	0.0085 ( 1.37 )	0.0006 ( 0.09 )	-0.0037 ( -0.53 )
$\beta_3$	0.1531 ( 2.04 )	0.1504 ( 2.04 )	0.1515 ( 2.04 )
$\gamma$		0.4855 ( 5.45 )	0.2892 ( 1.95 )
$\bar{R}^2$	0.016	0.056	0.019
$NT$	430	430	430

Notes: See notes to Table 2, Panel A for Panel A and Table 3 for Panel B. In panel B column 3, a constant is included because of the first-difference of the trend term.

Table 5: State-wide data, Census based

	(1)	(2)	(3)
<b>Panel A: Estimation results, model in levels</b>			
$\beta_0$	8.6884 ( 561.01 )	4.5095 ( 4.06 )	-1.9095 ( -0.77 )
$\beta_1$	-0.0449 ( -1.80 )	-0.0357 ( -1.75 )	0.0306 ( 0.64 )
$\beta_2$	0.1061 ( 7.46 )	0.0607 ( 3.45 )	0.0060 ( 0.41 )
$\beta_3$	0.0823 ( 2.00 )	0.0726 ( 1.98 )	0.0579 ( 1.91 )
$\gamma$		0.4407 ( 3.74 )	1.1134 ( 4.24 )
$\bar{R}^2$	0.144	0.314	0.819
$NT$	550	550	550
State dummies	NO	NO	YES
Time trend	NO	NO	YES
<b>Panel B: Estimation results, model in differences</b>			
$\beta_2$	0.0445 ( 3.11 )	0.0366 ( 2.56 )	0.0303 ( 2.07 )
$\beta_3$	0.0879 ( 2.01 )	0.0846 ( 2.01 )	0.0863 ( 2.01 )
$\gamma$		0.5153 ( 7.59 )	0.2473 ( 2.26 )
$\bar{R}^2$	0.073	0.125	0.041
$NT$	500	500	500

Notes: See notes to Table 2, Panel A for Panel A and Table 3 for Panel B. In panel B column 3, a constant is included because of the first-difference of the trend term.

Table 6: Treatment effect by year

	(1)	(2)	(3)
<b>Panel A: Tax-revenue based</b>			
<i>Levels model</i>			
$\beta_{3,1998}$	0.0624 ( 1.97 )	0.0548 ( 1.95 )	0.0423 ( 1.86 )
$\beta_{3,1999}$	0.1027 ( 2.02 )	0.0959 ( 2.02 )	0.0860 ( 2.00 )
$\beta_{3,2000}$	0.0915 ( 2.01 )	0.0864 ( 2.02 )	0.0807 ( 1.99 )
$\beta_{3,2001}$	0.0724 ( 1.99 )	0.0716 ( 2.01 )	0.0708 ( 2.00 )
<i>Difference model</i>			
$\beta_{3,1998}$	0.0129 ( 1.52 )	0.0119 ( 1.48 )	0.0125 ( 1.50 )
$\beta_{3,1999}$	0.1531 ( 2.04 )	0.1504 ( 2.04 )	0.1515 ( 2.04 )
$\beta_{3,2000}$	0.0195 ( 1.74 )	0.0155 ( 1.63 )	0.0170 ( 1.68 )
$\beta_{3,2001}$	0.0005 ( 0.11 )	0.0092 ( 1.37 )	0.0041 ( 0.77 )
<b>Panel B: Census based</b>			
<i>Levels model</i>			
$\beta_{3,1998}$	0.0605 ( 1.98 )	0.0493 ( 1.93 )	0.0289 ( 1.64 )
$\beta_{3,1999}$	0.0823 ( 2.00 )	0.0726 ( 1.98 )	0.0579 ( 1.91 )
$\beta_{3,2000}$	0.0762 ( 2.01 )	0.0686 ( 2.00 )	0.0581 ( 1.95 )
$\beta_{3,2001}$	0.0841 ( 2.03 )	0.0822 ( 2.03 )	0.0797 ( 2.04 )
<i>Differenced model</i>			
$\beta_{3,1998}$	0.0065 ( 1.99 )	0.0051 ( 1.94 )	0.0061 ( 2.01 )
$\beta_{3,1999}$	0.0879 ( 2.01 )	0.0846 ( 2.01 )	0.0863 ( 2.01 )
$\beta_{3,2000}$	-0.0144 ( -1.97 )	-0.0197 ( -2.04 )	-0.0159 ( -1.99 )
$\beta_{3,2001}$	0.0237 ( 1.83 )	0.0323 ( 1.91 )	0.0273 ( 1.86 )

Notes: Estimates for  $\beta_{3,1999}$  are repeated from Tables 4 and 5. Columns correspond to the models in Tables 4 and 5.