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

Chadi S. Abdallah
Assistant Professor of Economics
Farmer School of Business
Miami University
Oxford, Ohio 45056
abdallcs@muohio.edu

and

William D. Lastrapes
Professor of Economics
Terry College of Business
University of Georgia
Athens, GA 30602
last@terry.uga.edu

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Abstract

We estimate how spending in Texas responded to a 1997 constitutional amendment that relaxed severe restrictions on home equity lending. We use this event as a natural experiment to estimate the importance of credit constraints. If households are credit-constrained, such an increase in credit availability will increase their spending. We find that Texas retail sales at the county and state levels increased significantly after the amendement, lending support to the credit-constraint hypothesis. We confirm these findings and refine our interpretation of the estimated aggregate-level responses using household-level data on home equity loans.

Keywords: Difference-in-differences, credit constraints, housing markets, synthetic control
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Households that face binding constraints on their ability to borrow on future income – either through pure quantity limits or restrictive borrowing rates – suffer welfare losses because they cannot optimally allocate consumption over time. Whether such constraints exist, how extensive they might be, and how they distort behavior are questions that matter for many areas of study, including aggregate consumer behavior (Campbell and Mankiw 1989, 1990), 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), and the spillover of housing markets to overall economic activity (Iacoviello 2004, 2005).

Constraints on borrowing are most often the result of the high costs lenders bear to obtain information about borrowers' uses of funds. Collateralized debt, such as home equity loans, is a common way to mitigate these information costs and promote the efficient flow of funds through financial markets.[1] Legal restrictions on the ability of lenders to write home equity loans can therefore impose potentially binding constraints on intertemporal consumption decisions.

Prior to 1998, homeowners in Texas faced such legal restrictions on home equity loans. Until then, the Texas Constitution effectively prohibited home equity as collateral except for a very limited set of expenditures, primarily on housing. But the citizens of Texas amended their constitution, first in 1997 and again in 1999, to allow home equity lending for general, non-housing spending. At the time the 1997 amendment was enacted, no other state in the US restricted such loans for discretionary household spending as severely as Texas.

In this paper, we use the timing of this unique event in Texas as a natural experiment to estimate the response of aggregate household spending to an increase in the availability of credit, with the aim of helping to quantify the importance of credit constraints. If home-

[1] According to the US *Flow of Funds Accounts* (June 9, 2011, Table L.218) US households had almost $10 trillion in mortgage debt outstanding as of the first quarter of 2011, of which almost 10% was home equity lending and home equity lines of credit. Housing wealth comprised over 25% of total household assets (*ibid.*, Table B.100).

owners are not credit-constrained, and do not anticipate being constrained in the future or any state of the world, intertemporal consumption models predict that their spending will be unaffected by an increase in the availability of credit (although such households might use opportunities for new debt to pay down other higher-cost debt or to convert home equity into other assets). On the other hand, credit-constrained households spend less on current consumption than is optimal given their lifetime budget constraints, so their spending will rise when restrictions on borrowing are lifted. Even if households are not *currently* constrained, anticipation of *future* constraints can limit current spending and increase saving through precautionary motives.[2] The Texas amendment provides an opportunity to examine these predictions by comparing spending in the state before and after its passage.[3]

There is substantial evidence that credit constraints matter for consumer behavior, especially at the individual level. Most studies examining this issue gauge the role of credit constraints either by estimating how marginal propensities to spend out of expected cash flows vary across income or asset levels, or estimating the response of spending or borrowing to an exogenous relaxation of credit limits. The canonical permanent income hypothesis without credit or liquidity constraints predicts that marginal propensities should not vary with asset levels, and credit-availability shocks should have no effect on consumption paths. Papers such as Hurst and Stafford (2004), which examines the extent to which households use home equity to smooth consumption in the face of negative income shocks, Stephens (2008), which 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),

[2]See, for example, Zeldes (1989) and Gross and Souleles (2002) for a discussion of anticipated borrowing constraints and the precautionary motive for saving.

[3]The Texas constitutional restrictions prior to 1998, when the new law took effect, need not have prevented consumption smoothing using home equity. Even before the amendment, Texans could take cash out when refinancing current home mortgages for general spending purposes. Mortgages in Texas could have originated at higher loan-to-value ratios (thereby preserving cash for spending) in anticipation of home equity growth. And Texans could have been less likely to buy homes in the first place since the restrictions on home equity lending reduced the value of home ownership. To the extent that these alternatives were good substitutes for second mortgages for smoothing consumption over time, the sudden availability of such loans could lead simply to a re-balancing of portfolios. Thus, how aggregate spending in Texas responded to the legal changes is an empirical question worth considering in the context of the credit constraints literature.

which estimates spending responses to temporary tax rebates, fall into the first category. Work in the second vein includes Gross and Souleles (2002), which estimates the response of credit card debt to exogenous increases in credit limits, Yamashita (2007), which examines the propensity of households to take on home equity debt when house prices rise, and Leth-Petersen (2010), which estimates individual consumption and debt responses to a 1992 credit market reform in Denmark that, as with the 1997 Texas amendment, freed homeowners from constraints on the use of mortgage loans for general spending purposes. Each of these studies finds evidence consistent with the existence of credit constraints. They are less uniform, however, in the overall importance attached to such constraints.

Our paper falls into the second camp, and is most closely related to Leth-Petersen (2010) in terms of the nature of the credit-availability event. Our main contribution is to refine what we know about the importance of credit constraints by looking at an episode – the Texas constitutional amendment on home equity loans – that has yet to be carefully examined in this context. Quantitative studies like ours can help resolve the inherent uncertainties of measuring the importance of credit constraints by bringing to bear additional information. The Texas intervention is a particularly interesting case study in light of the presumed role of home equity lending as a cause of excessive debt accumulation and house price bubbles in the 2000's (see, for example, Mian and Sufi, forthcoming). Also, by the nature of the particular policy intervention, we can directly observe the behavior of the 'treated' group (Texans) and the 'control' group (non-Texans). Leth-Petersen (2010), for example, must make theoretical assumptions regarding which sets of households will be affected by the credit market reform (in his case, 'illiquid' households) since the change in Denmark's law applied to all its citizens.

Another distinction of our work is that we estimate the effects of the Texas credit reform directly on *aggregate* measures of spending, in particular at county and state levels, and not at the household level. One reason for this focus is the difficulty of obtaining sufficient spending data for our purpose, both in terms of quantity (numbers of observations in Texas) and quality (informative measures of non-housing consumption), at the individual level. But most importantly, aggregate effects typically are most relevant for policy decisions since they capture the overall effects of an intervention. Studies using micro data usually attempt to infer aggregate effects from the responses of individuals, as in Leth-Petersen (2010, p. 1101) and Gross and Souleles (2002, pp. 182-83), but we are able to estimate them directly.

Given this focus, our estimates of the amendment's effect on spending in section 3 below should be interpreted not as direct or partial equilibrium effects but as general equilibrium responses that capture spill-overs across units, multiplier effects as extra spending leads to increases in sellers' income and further rounds of spending, and spending that might result from the effects of the law on housing prices, other asset prices, and goods prices. These overall effects are of paramount importance here, but it is also worthwhile to separate the direct effects on spending financed by new home equity loans from the indirect effects. We attempt to do so, in the absence of household-level data on spending, by estimating the effect of the legal changes in Texas on home equity borrowing itself, from which we can infer a direct effect on spending. For this purpose we use household-level data on second-lien home equity loans in selected metropolitan areas from the American Housing Survey. We report our results from this analysis in Section 4 below.

In an ideal experiment, the 'treatment' or policy intervention would be assigned to individuals randomly. Random assignment ensures that the intervention is exogenous with respect to the endogenous variables of interest, appropriately defines treatment and control groups, and correctly identifies the effects of the intervention by comparing outcomes between the two groups. Clearly, we cannot suppose that the passage of the Texas amendment was a randomized experiment because the changes ultimately reflect the preferences of the citizens of Texas through legislative action and direct referenda. Instead, we base our inference on the assumption that the episode can be exploited as a 'natural experiment' – an incidental source of variation in credit availability across different groups that is exogenous to retails sales or unobserved determinants of sales (conditional on control variables). This assumption will fail if Texans "selected into" the treatment group based on expected changes in the demand for credit, due perhaps to anticipated income growth and the demand for spending, that we do not otherwise control for. To the extent that the exogeneity condition holds, we can consider the timing of the constitutional event as containing information about a credit-availability shock, and identify its effects. Otherwise, our estimates of the spending effects of the law will be biased upward relative to an ideal random experiment. In Section 1 below we discuss this and other identification issues in more detail, and argue for the plausibility of interpreting the Texas event as a natural experiment.

Our aim requires that we estimate a counterfactual that we cannot observe – the path of Texas spending had there been no amendment in the late 1990's. We use standard difference-in-differences and time-effects regression methods to estimate this counterfactual using variation in spending before and after the passage of the amendment and across treatment and control groups. Because this approach generally relies on arbitrary criteria for selecting the control group, we also estimate the amendment's treatment effects on spending using the "synthetic control" methods of Abadie, Diamond and Hainmueller (2010), which selects a counterfactual spending path for Texas systematically. Proper inference depends on choosing a control group that effectively mimics the behavior of Texas under the counterfactual scenario. These empirical methods are briefly described in Section 2 of the paper, including our use of 'placebo' methods to account for the uncertainty involved in selecting control groups.

Section 2 also describes the main data we use in the county and state analysis. Because overall consumption data *per se* are not available at these levels of aggregation, we follow a common approach and use retail sales as a proxy (e.g., Case, Quigley and Shiller 2005 and Garrett, Hernàndez-Murillo, and Owyang 2005). Besides being available for counties and states, retail sales data have the advantage of capturing *non-housing* expenditures, which are most relevant for our purposes. On the other hand, retail sales do not account for all categories of consumption and therefore ignore potentially relevant spending. Because retail sales measures transactions involving *sellers* within county or state boundaries, they only imperfectly capture the spending of *consumers* within those boundaries; some spending by Texans will be on goods purchased outside the state, while some sales within Texas will be to visitors from outside the state. Nonetheless, we expect retail sales to be a reasonable proxy for non-housing consumption, and how sales respond to the constitutional change is of interest in its own right.[4]

We find strong support for the relevance of binding credit constraints: retail sales in Texas, at both the county and state levels, increase significantly in response to Texas's lifting of home equity lending constraints. Our estimates imply that real per capita spending of the average Texas county is 2% to 4% higher in 2002 relative to what it would have been without the change, and that this response is even larger for low income counties and counties with high home equity before the event. At the state level, spending is up to 7% higher by the year 2001, although the average effect post-amendment has the same range as the average county. Our estimates of the direct effects from the household data suggest that per capita spending would have risen by between 0.27% and 0.36% after the amendment, a range that encompasses Leth-Petersen's (2010) aggregate estimates for the Danish natural experiment of 1992.

We see our results as providing support not only for the importance of credit constraints generally, but also for the housing "collateral channel" as the main mechanism by which housing markets and prices affect the wider economy, support that is consistent with the recent work of Iacoviello (2004, 2005), and Iacoviello and Nehri (2010). The conventional view that declines in house prices make homeowners feel poorer and therefore reduce spending via a pure 'wealth effect' is theoretically flawed (Buiter 2008) and has little empirical support. Here we exploit a change in housing market institutions that is unlikely to directly cause a large wealth effect, but is surely related to credit conditions.

[4]At the national level, retails sales comprise almost half of all personal consumption expenditures, the latter of which includes imputations for housing expenditures. In 2002, the latest date in our sample, retail sales in the US were about $3.5 trillion and personal consumption expenditures were $7.5 trillion.

1 The Texas Constitutional Amendment

1.1 Homestead protection in Texas

Legal restrictions on lending in Texas date back to well before the territory's establishment as a US state in 1845, when many settlers "came to Texas with the hope of leaving debts permanently behind them..." (McKnight, 1983, p. 375). In 1829, to protect his colonists from foreign (i.e., American) creditors, Stephen F. Austin prompted the legislature of the Mexican state of Coahuila y Texas to enact Decree No. 70, which extended to the Texas colony the Spanish legal principle of exempting "vital property" from creditors. Although this decree was repealed in 1831, the principle of homestead protection found its way into the Texas Homestead Act of 1839, passed in response to the Banking Panic of 1837 to protect citizens from forced sales of their homes, and into the state's original constitution in 1845 and its subsequent versions. Ever since, the sanctity of the homestead has been a basic right of Texas citizens.[5]

Article XVI, Section 50 of the Texas Constitution of 1876, the fifth version of the document since statehood, protected homesteads from foreclosure except for nonpayment of the original loan to purchase the home (including property taxes), or for debt incurred to finance home improvements. Because this restriction on mortgage lending was embedded in the state's constitution, it has been difficult to relax, despite nationwide innovations in home equity lending available to citizens in the other 49 states.[6] From 1876 to 1997, Section 50 has been amended only twice, extending homestead foreclosure protection to single adults in addition to families in 1973, and then in 1995 exempting from protection debts incurred when buying an undivided interest in the homestead (usually as a result of a divorce set-

[5]For a history of Texas's role in the evolution of homestead protection laws in the US, see McKnight (1983).

[6]Constitutional amendments in Texas require a joint resolution from both the state Senate and House of Representatives, adopted by a two-thirds vote of the membership of each, and passed by a majority of the state's voters.

tlement or inheritance) or paying off federal tax liens on the property.[7] Although the 1995 amendment relaxed constraints on the use of the home as collateral, the aim was to help individuals *maintain* the homestead in the face of very specific (usually adverse) circumstances, and were thus limited in scope. Home equity loans for general purpose spending remained prohibited by law.

In December 1994, the Texas Senate Interim Committee on Home Equity Lending came out in strong support of relaxing the constitutional restrictions on lending. Instead of suggesting a complete repeal of the constitutional homestead protection, the committee's final report recommended explicit exceptions to the restrictions on foreclosures while incorporating specific limits to home equity lending to protect consumers, and called for the state legislature to put its amendment proposal on the ballot for voters to decide. Although the proposal did not receive the needed two-thirds support in the state House of Representatives in 1995, it passed the Senate, the first time ever such a proposal made it that far in the amendment process.

The committee's report ultimately culminated in *House Joint Resolution 31*, which 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." The joint resolution was passed by the Texas House and Senate in May 1997, and the resulting amendment proposal (Proposition 8) was approved by Texas voters on November 4, 1997 with almost 60% of the 1.17 million votes. The approved amendment to Article XVI, Section 50 of the constitution, effective January 1, 1998, allowed traditional home equity loans (but not home equity lines of credit), up to a total loan-to-fair-market-value of 80%, without restriction on how the proceeds were to be used. It also authorized 'reverse mortgages', which give house-rich but cash-poor borrowers, typically the elderly, the opportunity to receive fixed monthly payments over a given period in return for sacrificing equity in the home at the

[7]See *Amendments to the Texas Constitution Since 1876*, Texas Legislative Council, May 2010, p. 102. Liens attached by the federal government for tax purposes are not enumerated as an exception to foreclosure in the Texas constitution, but federal law trumps the state restrictions.

time of the owner's death or the home is sold or vacated. With this new constitutional law, Texas citizens finally had the right to extract equity from their home, without completely refinancing an original mortgage, for current spending.[8]

However, uncertainties in the amendment regarding both traditional home equity loans and reverse mortgages potentially hindered such lending in the year following its passage. First, at the time of the amendment the constitution's definition of a homestead limited urban households to no more than one acre of land. This limitation prevented homeowners living on lots exceeding one acre from participating in home equity borrowing. Second, inexperience with the nature of reverse mortgages led to serious distortions in this market. For example, the new law allowed borrowers of reverse mortgages to avoid repayment simply by informing lenders of their new address after a move. In addition, the amendment was inconsistent with federal rules on reverse mortgages, making such loans ineligible for purchase by Fannie Mae and hindering their value in secondary markets. These shortcomings were resolved with the passage of two additional constitutional amendments on November 2, 1999.[9]

The turn of the century saw further tinkering with the home equity laws. There were additional amendments to the state constitution in 2001, 2003, 2005 and 2007, mostly making minor changes and clarifications to the existing laws on home equity lending. However, the most significant change was Proposition 16, approved by voters with over 65% of the vote in November 2003, which legalized home equity lines of credit.[10] Because our focus in this paper is on the 1997-99 period, which arguably captures the biggest regime shift, we leave consideration of the effects of the 2003 amendment for future work.

[8]The appendix below gives an excerpt of the relevant parts of the Texas Constitution and the amendment of 1997.

[9]Details of these issues are discussed in the Interim Committee on State Affairs Report to the 76^{th} Legislature, November 2 1998, and the Texas House Research Organization Focus Report on Amendments Proposed for the November 1999 Ballot. Proposition 2 on the November 1999 ballot amended Article XVI, section 50 to clear up the issues with reverse mortgages, while Proposition 6 amended XVI, section 51 by increasing the acreage limit used to define urban households to 10 acres. The former proposition passed with 64.2% of the vote, and the latter passed with 67.5% of the vote.

[10]For details of all of these legislative developments, see *Home Equity, Reverse Mortgage, and Home Improvement Lending in Texas: Information and Resources*, Finance Commission of Texas (fc.state.tx.us/homeinfo/homeindex.htm)

1.2 The amendment and identification

Is the effective date of the amendment – January 1, 1998 – appropriate for identifying the amendment's effects on spending? If the new rules regarding home equity loans were anticipated by Texas citizens prior to the effective date, spending could have responded earlier. Before 1997, public discussion on relaxing lending constraints had been going on for a while, so it is difficult to make the case that citizens were unaware of proposals for change. However, actual passage of the law remained uncertain, given strong views on both sides of the issue, at least until May 1997 when the Senate and House passed the resolution. Indeed, as noted above, an earlier proposal to allow home equity loans failed to gain legislative support in 1995. Additionally, the details and extent of reform were unclear until passage, with the Texas House, hesitant to be perceived as weakening homestead protections, floating proposals with very limited changes. For example, one House proposal allowed second mortgages only for college tuition or medical bills (Stutz 1997). The likelihood of anticipated spending effects before 1998 is thus small. In any case, the unexpected nature of the intervention is not as important here as it is for the Danish natural experiment of Leth-Petersen (2010, p. 1083), who relies more heavily on this assumption then we do for identification.

As with any case study of treatment effects or program evaluation with 'before' and 'after' comparisons, our estimates are subject to being mis-identified because of events in Texas affecting retails sales that were coincidental to the 1997 amendment and not controlled for in our empirical models. While it is not possible to account for all contingencies, we have checked two obvious potential sources of change in Texas that could confound our identification of the effects of the home equity laws. First, there were no major changes in sales tax rates in the state during this period. The state sales tax rate of 6.25% has been in effect since 1990. Under half of counties in Texas impose additional sales taxes, but these are typically small, like half a percent. We found no significant reductions in these tax rates in the late 1990's. Hence, there is no evidence to suggest that lower sales tax rates in Texas around the time of the amendment might influence our results. We are also able to directly control for changes in sales taxes and differences across states in the regression analysis.

Second, we took a close look at the other amendments that were passed at the time of the home equity changes.[11] Of the other 14 propositions in 1997 and 15 in 1999, only three that passed can plausibly be related to retail spending. Proposition 1 in August 1997 and Proposition 2 in November of that year increased the homestead exemption by \$10,000 and limited increases in appraised home values, both of which effectively lowered property tax burdens to households. However, the increase in the homestead exemption was predicted to lead to only a small reduction in property taxes for the average homeowner; and more importantly, the loss in property tax revenues to local school systems was replaced by general state revenues. Thus, the amendment was less a *reduction* in overall tax burdens than a *reallocation* of taxes, which would likely not lead to overall spending increases. The limit on increasing home value appraisals likewise should not have increased general spending since appraisal limits simply spread the effects of rising home prices on property taxes over time, with no substantial effect on reducing tax burdens. Proposition 12 in 1999 exempted vehicles leased for personal use from *ad valorem* taxes. This amendment probably increased the demand for leasing versus buying automobiles, but is very unlikely to have had noticeable effects on overall sales.

Finally, what of our claim that the Texas episode can serve as a natural experiment? To determine the plausibility of this claim, we have carefully reviewed contemporaneous accounts of the debate leading up to passage of the amendment. Our reading of these accounts suggests that the impetus for change ultimately came from the desire of Texans to have what citizens of other states had in terms of financial innovations, for both practical and philosophical reasons, regardless of perceived trends in income or the demand for credit. In particular, three "exogenous" factors driving the changes seem to have been prominent.

[11]See ballotpedia.org/wiki/index.php/List_of_Texas_ballot_measures

First, the US Tax Reform Act (TRA) of 1986, which eliminated income tax deductibility of all forms of consumer interest except mortgage interest, provided a major impetus for change. For obvious reasons, home equity lending expanded throughout the US after the 1986 law, as Texans looked on in envy.[12] As early as December 1986, as efforts to amend the constitution began to be renewed from previous attempts in the 1970's, this argument showed up prominently in contemporary statements of support.[13]

Second, the U.S. Court of Appeals for the Fifth Circuit ruled in April 1994 in a suit brought by First Madison Bank and Beneficial Texas Inc. that federal regulations from the Office of Thrift Supervision superseded the Texas constitution, thereby overturning the state's restrictions on home equity laws (Stutz 1994). Subsequent action by the US Congress later that year, effectively writing Texas's foreclosure protections into federal banking laws, quickly re-established the restrictions so that no opportunities for home equity loans arose. However, the appeals court decision accelerated local efforts to amend the constitutional restrictions because it brought attention and publicity to the issue and because Texas legislators wanted to ensure consumer protections and safeguards in case the Fifth Circuit's decision was upheld on appeal.

Third, the political environment in the mid-1990's was ripe for change. As noted in the Interim Senate Committee's 1994 report (p. 5) regarding home equity lending reforms, "the arguments pro and con ultimately boil down to philosophical issues: those in favor of them believe in less government regulation and the freedom of homeowners to take responsibility for their borrowing decisions, while those against them believe it is the duty of government to protect citizens through regulation." The Republican party in Texas was more aligned with the former position than the latter, and it gained power during this period. In 1997

[12] Transplanted Texans from other states, in particular, many of whom would have had first hand experience with the tax benefits of second mortgages, were well aware of the lost opportunity.

[13] For example, "proponents of a change in the [Texas] homestead law say the new federal tax law [the TRA] provides another good reason for some modification [in the state constitution]. Beginning next year interest on consumer loans will no longer be deductible, while interest on first and second mortgages secured by residences will be" (LaFranchi 1986).

the Republicans became the majority party in the state senate for the first time in a long while (Legislative Reference Library of Texas). Then-gubernatorial candidate George W. Bush is on record as favoring an amendment allowing home equity lending restrictions with sufficient consumer protections (Lucas 1994) and later, as governor in 1997, praising the legislature for bringing the issue to voters.[14] In addition, bankers groups, such as the Texas Bankers' Association, were heavily in favor of change. Although Texans have always prided themselves in the prominence they have given to the sanctity of the homestead, by the mid-90's the political tide had turned, finally making it possible to relax long-standing traditions on homestead protections in favor of greater economic freedom.

Although surely other factors were at play, these three undoubtedly played a major role in driving the credit market reforms of 1997 in Texas. None of these factors – the federal scope of the Tax Reform Act of 1986, the unexpected judicial decision that brought home equity restrictions to the front pages, or the salience of political philosophy during the debate – support the idea that local credit demand shocks drove the changes. They do, however, add plausibility to our interpretation of the event as, largely, a source of exogenous variation in credit availability.

2 Empirical methods and data

Given that the amendment can be interpreted as a natural experiment and that the timing of the event is well-defined, we can estimate the effects of relaxing credit constraints by comparing Texas spending before and after its passage to that of a relevant control group, which we interpret as the counterfactual. Here we describe the empirical methods that we use, as well as the balanced panel data for retail sales at the county and state levels.

[14] "Bush said a bill creating a statewide water conservation and management plan and a constitutional amendment to let Texans borrow against the equity in their homes were two of the session's major accomplishments." Robison (1997).

2.1 Regression models and synthetic controls

Let c_{it} denote the natural log of real spending per capita for either state or county $i = 1, \ldots, \nu$ at time $t = 1, \ldots, T$, where ν equals the number of counties (n) or states (N), depending on the data set. The general time-effects regression model we use to estimate the spending effects of the Texas event is

$$c_{it} = \sum_{h \neq t_b}^{T} (\alpha_h + \beta_h z_{it}) S_{TX,i} \tau_{ht} + \sum_{s \neq TX}^{N} \phi_s S_{si} t + x_{it}\gamma + \theta_i + \epsilon_{it}, \tag{1}$$

where S_{si} takes the value 1 if $i = s$ for the state sample or if county i resides in state s for the county sample and 0 otherwise ($s = 1, \cdots, N$, with $s = TX$ for Texas), τ_{ht} is standard time dummy set to 1 if h corresponds to year t and 0 otherwise, t is a linear time trend, z_{it} is a scalar variable, x_{it} is a vector of control variables, and θ_i is a vector of unobserved fixed individual state or county effects. We normalize the time effects on the base year t_b, so the term in parentheses gives the change in spending in Texas (the treated group) from the base year to year h, relative to non-Texas states (the control group), and as a function of the exogenous process z_{it}. We assume the base year is the year prior to treatment ($\tau_b = 1997$), so this term provides the basis for our estimates of and inference about the amendment's spending effects. In particular, we compare the level and dynamics of spending in Texas after the amendment in 1997 to spending before the amendment. If consumers are not credit-constrained and the amendment has no effects on non-housing expenditures in Texas, estimates of α_h and β_h, for $h > 1997$, will differ from zero only because of sampling error. The second term in (1) controls for state-specific time trends, while x_{it} controls for observed factors affecting spending that may be correlated with the timing of the amendment's passage.

In addition to equation (1) we estimate a restricted version of the model that averages over pre- and post-treatment periods, thereby ignoring annual variation in spending. If we assume $\alpha_h = \phi_{TX} h$ and $\beta_h = 0$ for $h \leq t_b$, and $\alpha_h = \alpha + \phi_{TX} h$ and $\beta_h = \beta$ for $h > t_b$, then

(1) collapses to

$$c_{it} = (\alpha + \beta z_{it}) S_{TX,i} \tau_t + \sum_{s=1}^{N} \phi_s S_{si} t + x_{it}\gamma + \theta_i + \epsilon_{it}, \quad (2)$$

where τ_t is 1 if $t > t_b = 1997$ and 0 otherwise.[15] When $\beta = 0$, the parameter α is a standard difference-in-differences estimator (Wooldridge 2002, p. 130): the difference between the average change in spending in Texas from before to after the amendment and the average change in other states, conditional on control variables and state-specific trends. β allows the effect to depend on z_{it}. While the general model provides a richer picture of the dynamics of spending behavior in Texas, both models control for state-specific and time-specific effects that might confound identification of the response to the constitutional amendment.

For each model and sample, we control for unobserved heterogeneity across states or counties (θ_i) by using the standard fixed-effects ('within') estimator and pooled OLS on the first-differences of the variables.[16] Standard errors used for assessing statistical significance are clustered at the state level, robust to heteroskedasticity and autocorrelation, and based on the non-parametric block bootstrap procedure suggested by Bertrand, *et al.* (2004, p. 265-67).

As with all difference-in-differences models, ours rely on a set of control units – non-Texas states or counties that have not been treated by the policy intervention – to account for the unobserved counterfactual – how Texas spending would have behaved in the absence of the constitutional amendment. The ideal control group should be similar to Texas along dimensions that matter for aggregate retail spending. The control group in the difference-in-differences approach is essentially the average of all non-treated units included in the regression model, where each included unit has the same weight. We consider two potential control groups in our analysis, all available non-Texas states and all non-Texas states without changes in sales tax rates during the estimation period. We assign weights, in effect, of zero to states excluded and weights of one to states included in the control group. To the extent that

[15]Note that $\tau_t = \sum_{h=t_b+1}^{T} \tau_{ht}$ and $t = \sum_{h=1}^{T} h\tau_{h,t}$.

[16] For detailed discussion of these estimators, see Wooldridge (2002, sections 10.5 and 10.6)

these weights do not produce a control group that is sufficiently similar to Texas, conditional on the control variables, our estimates may be biased.

We therefore provide additional estimates of treatment effects using the synthetic control method of Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010), which systematically select weights for the control group from a potential 'donor pool' to construct the counterfactual. This approach generalizes the difference-in-differences regression model not only by optimally selecting weights of the control group according to a well-specified criterion, but also by controlling for unobserved effects to be fixed over time as well as over individuals (*ibid.*, p. 495).

Define retail spending of Texas's synthetic control, or its counterfactual spending path, as

$$\tilde{c}_{TX,t} = \sum_{s \neq TX}^{N_d} w_s c_{st}, \quad t > t_b, \tag{3}$$

where $\sum_{s \neq TX}^{N_d} w_s = 1$ and N_d is the number of units in the donor pool. The synthetic control is thus a weighted average of spending of the donor pool units over the post-treatment period. Abadie, Diamond and Hainmueller (2010) suggest choosing the weights to minimize the distance between predicted sales in Texas during the pre-treatment period, conditional on a set of predictor variables. They show that, using these optimal weights in (3), $\alpha_{sc} = c_{TX,t} - \tilde{c}_{TX,t}$ for $t > t_b$ is an unbiased estimator of the treatment effect. In addition to reporting estimates of α_{sc} below, we use their 'placebo' testing strategy to conduct inference about the estimations. In particular, we construct estimates of the pseudo-treatment effects for each state in the control group, as if each of these other states faced a similar event. The distribution of these estimates can help us compare the magnitude of Texas's estimated effects to those for a randomly selected state, or more specifically, to control for uncertainty about how well the control group reproduces the counterfactual.[17]

[17] Abadie, Diamond and Hainmueller (2010) nicely describe the approach and provide software to implement it (mit.edu/ jhainm/synthpage.html). Their paper is also worth a look because it applies the techniques to a question similar to ours: how the passage of an anti-tobacco law in California in 1999 affected cigarette sales. The synthetic control method is an effective way to deal with estimation uncertainty when the size

2.2 Retail sales data

Our objective is to estimate the effect on overall non-housing consumption of the change in the home equity lending provisions of the Texas Constitution. This aim requires data on total spending aggregated across individuals. Given the nature of the policy intervention, aggregation at county and state levels is a natural choice. State-level aggregation gives a broader picture, but county-level data allow us to consider how aggregate effects might vary with county characteristics.

We assume that retail sales are a reasonable proxy for such consumption. The two series are closely correlated over time at the national level, and account for *non-housing* expenditures, a plus for our study. As previously indicated, sales will inappropriately include transactions by out-of-county or out-of-state consumers, but systematic differences (based, for example, on the relative importance of tourism) can be controlled for in our regressions below with county- and state-specific fixed effects.

The county-level retail sales we use are based on survey data from the Economic Census, Retail Trade Series, for establishments with payroll.[18] Sales figures are annual, but available only at five-year intervals; we have data for 1992, 1997 and 2002. 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 values 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 the treatment group, one state in our case and theirs, is fixed. Conley and Taber (2009) provide another approach for this situation.

[18]Details and sources for all data are provided in the appendix.

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 sales data because in part it redefines extant industries, identifies new industries, and alters how these industries relate to retail sales.[19] For this reason we exploit two county-level samples. When we estimate the time-effects model and the differences-in-differences model using the complete sample of 1992, 1997 and 2002, we scale the reclassified sales data in 1997 and 2002 to be comparable to the SIC system.[20] Because this scaling strategy may not perfectly account for the classification change's effects on sales, we also provide estimates from a limited sample that drops 1992, so that we can use only the NAICS-based measures of sales in 1997 and 2002. We choose 2002 as the final year in our sample to avoid confusing the effects of the 1997-99 Texas amendments with the 2003 approval of home equity lines of credit, and to avoid the potentially complicating effects of the housing price bubble of the early to mid 2000's. Of the 3143 counties in the US during this period and 254 in Texas, we include in our sample the 3006 and 244 counties, respectively, that have complete records for both sales and income for these years. Thus, we have up to 9018 county-level observations in our balanced panel.

For the state-level analysis, we might aggregate the county-level sales data to the state-level, but doing so would limit the data to a five-year frequency, and annual variation could be important. Furthermore, direct surveys of annual sales at the state level are scarce and unreliable.[21] Therefore, as has been done in other studies (e.g. Garrett, Hernàndez-Murillo,

[19]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 census.gov/eos/www/naics/.

[20]We use a 'bridging' factor computed by the Census Bureau that constructs sales under both classifications by state for 1997. We assume this state-wide factor holds across counties and for the year 2002.

[21]The *Statistical Abstract of the United States* reports annual retail sales data as compiled by *The Survey*

and Owyang 2005) we use as a proxy for annual retail sales for each state the ratio of state sales tax revenues to sales tax rates. The former are "general sales and gross receipts" collected from the Census Bureau's *Annual Survey of State Government Tax Collections*, while general sales tax rates are available from the Tax Foundation. The revenue data are reported on a fiscal year basis, so we convert sales to a calendar year period by pro-rating based on the month when the fiscal year ends.[22] The state sample is limited to 45 states because Alaska, Delaware, Montana, Oregon, and New Hampshire have no state-wide sales tax. Sales by this measure are in no way susceptible to bias from the 1997 changes in industry classification, so we are comfortable in using the sample period 1992 to 2002. The balanced panel thus contains 495 total observations.[23]

Sales imputed from tax revenues and rates may lead to biased estimates of actual consumer behavior if goods and services exempted from sales taxes vary systematically across states and time. For the question at hand, our regression models can control for these differences if they are state-specific or follow a steady time trend. However, large changes over time in categories of exempted goods, especially around the time of the amendment, could affect our results. In 1997, the Texas legislature made just one change in the state's sales tax laws, repealing a court-mandated expansion of exemptions to manufacturing goods. The projected tax revenue increase for 1998 from this change was $56 million, or less that one-half of one percent of overall sales tax revenues for the year (Zelio 1998, Appendices G and H).

of Buying Power Data Service: Market Statistics. However, these data are not adjusted for the industry re-classification noted above, and are thus susceptible to unknown measurement error and bias. They also do not include (inexplicably) observations for 1999, a crucial year for our study.

[22]For a state with a fiscal year ending on June 30, 1998, say, its calender year sales for 1998 are the sum of half its sales in fiscal year 1998 and half its sales in fiscal year 1999. The Census Bureau also reports historical data in their *Quarterly Summary of State & Local Tax Revenues*, which can more precisely be converted to a calendar year basis. However, according to staff economists at the Census Bureau, these quarterly estimates are highly preliminary and much less reliable than the independently conducted annual survey. We therefore rely on the annual survey data.

[23]By restricting the state sample period to begin in 1992, we 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, as we've noted above, 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. Using 2002 as the final year in 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.

According to this source, other states reported minor changes in exemptions for some goods, but all were projected to have similarly small revenue effects.

We can also think more generally about measurement issues by comparing our retail sales data at the state and county level for years in which there is overlap. Since they are based on survey data and not tax-based imputations, our county-level data do not omit retail trades that are tax exempt. On the other hand, the county data do not account for sales of firms without payrolls, whereas the tax-imputed series do. Any major problems along these lines should be evident in large differences in the data between our sources. As a check, we sum county-level sales by state for 1992, 1997 and 2002, do the same for population, compute real per capita sales state-wide implied by the county data, and compare to the state figures for these years. For some states, the differences can be large.[24] However, in almost every case the change *over time* is small – less than 5% (in Texas, less than 2%), so that these differences will be evident only as fixed individual effects. Overall, we are confident that mis-measurement issues with our data do not drive our results.

The top panel of Figure 1 compares log real per capita retail sales in Texas to log real per capita sales of the average non-Texas state (excluding the five states without sales tax) annually over the 1992-2002 period. A relatively large increase in spending in Texas is evident immediately following the passage of the amendment. From 1997 to 1999, sales per person grew on average 1.5% faster in Texas than elsewhere. On the other hand, when we take average sales from 1992 to 1997 and 1998 to 2002, Texas sales decline relative to the other states, largely due to the large decline in Texas sales in 2002. We find similar patterns for sales relative to income, plotted in the second panel. Although relative sales decline for both groups over much of the period, the decline is smaller for Texas immediately following the amendment. In the next section, we report and discuss our formal analysis of these data.

[24]The most extreme case is Hawaii, for which tax-imputed sales are twice the payroll sales, likely because of substantial tourist sales and the fact that Hawaii allows no exemptions from its general excise tax.

3 Estimation results

3.1 State-level estimates

Table 1 reports our estimates of the amendment's treatment effect on retail spending using the annual state-level data over the period 1992 to 2002. The estimates are based on the regression models in equations (1) and (2), as well as the synthetic control counterfactual in equation (3). Because cross-state variation is less informative about how the treatment effect might depend on other factors, compared to counties, we assume a constant treatment effect in the state-level regressions: $\beta_h = 0$ for all h. We report for the time-effects model (1) the coefficients α_h, $h \neq 1997$ (i.e., we set $t_b = 1997$ to normalize on the pre-treatment year), while for (2) we report α, the standard difference-in-differences estimator. The vector of predetermined control variables, x_{it}, includes the log of per capita real disposable income, the state sales tax rate, the log of real house prices, mortgage interest rates, and real oil prices. The rationale for including housing prices and mortgage rates is to control for variation in housing markets across states and time that might affect spending. For example, while Texas citizens could not use home equity loans to finance non-housing consumption until 1998, they *could* refinance the original mortgage, with perhaps a higher loan-to-value ratio, to pay for such spending. Thus, higher house prices and lower mortgage rates could lead to higher retail sales through a "re-finance" channel. Oil prices may affect spending positively as a source of wealth for states like Texas that rely heavily on the oil industry, or negatively through a reduction in wealth for states that are net consumers of oil.[25] We report results from fixed-effects (FE) estimation and pooled estimation after first-differencing (FD) for two control groups – all the other 44 US states with general sales taxes, and a reduced sample of

[25] An additional consideration is variation across time and states of bankruptcy laws. In many states, equity in the homestead is protected up to a limit; if equity exceeds this limit, forced sale is allowed to pay off debtors. Home equity can therefore shield the homestead from foreclosure by reducing equity in the home. Homesteads in Texas are almost completely exempt from bankruptcy proceedings, so there is no reason to expect the amendment to have altered bankruptcy decisions there. For our control samples, we found no obvious changes in bankruptcy laws around the treatment date that might have affected the demand for home equity loans, and therefore retail spending, in those states.

36 states that eliminates those for which general sales tax rates changed during the period. Robust standard errors are in parentheses.

We find that spending in Texas increases significantly after the amendment was enacted relative to the control groups, which is consistent with the hypothesis that the new Texas law significantly relaxed borrowing and spending constraints. For the time-effects model, the most conservative estimates are in the first column, which reports fixed-effects estimates for the full control group. Relative to the year prior to treatment (1997), real per capita sales in Texas were 2% higher in 1998, over 4% higher in 1999 and 2000, 5.6% higher in 2001, and 2.6% higher in 2002, compared to counterfactual spending in Texas had there been no amendment. These estimates are quantitatively important and, except for 2002, statistically different from zero at typical test sizes. Although spending was also higher in Texas before 1997 relative to controls, the magnitudes are no bigger than half the largest effects after the amendment. Interestingly, the measured effect is relatively small in 1998 but picks up in 1999 and 2000, which suggests that the data are consistent with contemporary accounts that home equity lending did not rise immediately because of caution, learning costs, and the ambiguities leading up to the clarifying amendments of 1999. The estimated responses for the first-differenced estimation and the reduced sample follow the same dynamic pattern as those in the first column, but are larger and more precise. The spending effect in 1998 is relatively large (over 3% in each case), while the largest estimate is 7.6% in 2001 for the fixed-effects estimator in the reduced sample. The top four panels of Figure 2 plot the time-effects coefficients, with approximate 90% confidence intervals, to better illustrate the dynamic patterns in the data.

The statistically significant positive effects also hold for the restricted difference-in-differences model, albeit at smaller magnitudes. The effects are robust across models and control samples, with α estimates ranging from 2.2% to 3.2%. In all regressions, our results are also robust to adding variables like population to the control vector and altering the composition of x_{it} by eliminating some variables, although the magnitudes of the estimated treatment effects tend to rise in some cases. Controlling for state-specific trends, however, is important for our findings.

For the synthetic counterfactual analysis, we alternatively use as potential donor pools the same two control groups as above. We construct a 'synthetic Texas' by choosing the linear weights on states in the donor pool (w_s) to closely match retail sales predictor variables averaged over the pre-treatment period. Those predictor variables are the control variables in x_{it} described above and the log of Texas real per capita sales in each of the years 1992 to 1996.[26] Table 2 contains estimated treatment effects as measured by α_{sc}, the difference between the log of actual real per capita spending in Texas and its synthetic control as constructed in (3), while the bottom two panels of Figure 2 plot these differences. Figure 3 plots the paths over the sample period of log real per capita sales for both actual and synthetic Texas.

As seen in the figures, synthetic Texas using the reduced donor pool tracks actual Texas very closely before the amendment, but not afterwards as actual spending rises above its counterfactual from 1998 to 2001. The measured effect in 1998 is very similar in magnitude to that estimated in the first-differenced regressions, but the effects become smaller than implied by the regression models in the following years. Indeed, the synthetic approach suggests the effect of the amendment completely disappears in 2002, and that the effect in 2001 is relatively small. On the other hand, the average synthetic control effect over the period 1998 to 2002 is 2.1%, conceptually the same as and very similar in magnitude to the difference-in-differences estimates. For the larger donor pool, synthetic Texas systematically over-predicts actual Texas in the pre-treatment period. But if we reduce the post-treatment gaps by this bias (0.008), the treatment effects for the two control groups are very similar. The primary difference is that the treatment effect is larger in 2001 and 2002 for the larger donor pool, and therefore more similar to the regression results.

[26]For the larger donor pool, the procedure gives all states except Maine non-zero weight in the synthetic. For the reduced donor pool, only four states have non-zero weight, but this is not unusual: in Abadie, Diamond and Hainmueller (2010), the synthetic involves only five states.

To speak to the statistical significance of the synthetically estimated effects, we perform a 'placebo' test similar to that in Abadie, Diamond and Hainmueller (2010, pp. 501-03) – we repeat the construction and comparison of synthetic controls to actual data for each of the states in the donor pool; that is, we imagine that each of those states, alternatively and instead of Texas, enacted a similar law to Texas in 1998, and compare the Texas effect to the distribution of these estimates. If the measured effects for Texas are large compared to this distribution across the non-treated states, then our findings for Texas are 'significant,' and not likely due to chance.

We examine the distribution of estimated treatment effects in two ways. First, Figure 4 plots the estimated gaps between the treated state and the synthetic state (α_{sc}) for Texas (thick black curve) and all the other states in the reduced donor pool for which the mean square error between sales and its projected value in the pre-treatment period does not exceed 20 times that of Texas, a total of 15 states.[27] We see that at least for 1998, 1999 and (perhaps) 2000, the treatment effect for the actual policy intervention in Texas is large relative to the distribution across states not having an intervention. In 2001 and 2002, we cannot say with confidence that the effect differs from zero according to this metric. Second, for Texas and each of states in the donor pool (without exclusions for pre-treatment fit), we compute the ratio of the mean squared error between treated and synthetic post-treatment to pre-treatment, which can more precisely control for relative differences in fit of the synthetic controls. For the reduced donor pool, the ratio in Texas is large (73.1) and only one state (South Carolina, with a value of 79.7) has a larger ratio than Texas, providing further support for the significance of our findings.[28]

In sum, we find convincing evidence at the state level that the Texas home equity amendments in the late 1990's relaxed borrowing constraints and increased retail spending in the

[27]By excluding states for which the synthetic control fits poorly in the period prior to treatment, we reduce the chance of observing large post-treatment effects caused by poor fit.

[28]Texas falls just within the top third of this distribution for the full donor pool, so statistical significance in this case is somewhat weaker.

range of 2% to 7%, at least for the two or three years following passage of the amendment. The effect appears to be persistent, but not permanent, given the declines in 2002. The magnitudes of these reduced form, aggregate affects are both plausible and economically relevant.[29]

3.2 County-level

We turn now to the results for the county-level data. One advantage of using data at this level of aggregation is that we can observe variation *within* the treated group (Texas), so that we might discover how treatment effects vary across time and county-wide characteristics.[30] In this section, therefore, we consider regression models relaxing the restriction that β_h is zero for all h, in addition to those restricting the effects to be constant. A disadvantage of these data is that we lose the dynamic richness of the annual state data since our observations occur only at 5-year intervals. We are thus unable to isolate specific year effects for all years with the county-level observations. Also, the synthetic control approach is less appropriate for the county data because the method can only be applied to a single treatment group, whereas over 240 counties were treated here, and because of the paucity of pre-treatment observations.

As we note above, using the sales data scaled to the SIC allows us to use all three time series observations, in which case we have 9018 county-year observations. For this sample, we estimate both the unrestricted time-effects and restricted difference-in-differences regression models. For the limited sample with two time observations (1997 and 2002), the time-effects model collapses to the difference-in-differences model. We consider three control groups: a) all-states (here, all 49 other US states); b) only states without changes in sales taxes

[29] Although they are lower than some contemporary projections – Duca, Gould and Taylor (1998, p. 9) cite estimates from a University of Texas study suggesting at the time "that the temporary stimulus [of the home equity act] could be equivalent to a 9-percent increase in Texas retail sales." We have not been able to locate the original study.

[30] A smaller advantage is that we have data at the county-level for all 50 states.

during the period; and c) nine states that match Texas along three dimensions: state-wide importance of oil production (states, like Texas, in the upper 25^{th} percentile of annual per capita oil production in 1997), level of house values (states, like Texas, in the lower 25% of the real house price distribution in 1997), and whether the state borders Texas.[31] We add this third set as a plausible control since we are not able to formally construct the synthetic control group. We include the log of real per capita income and τ_t in x_{it}; given the presence of state-specific trends, our results were robust to including other pre-determined controls such as population and the unemployment rate. We report the results in Table 2, including estimates from first-difference estimation only because fixed-effects and first-differencing are identical for the two period case, and because the results are similar for the three period sample.

When we restrict the effects to be constant (Panel A), the estimated county-level responses are precisely estimated and generally similar in magnitude to those at the state-level. For the difference-in-difference model using the full time sample, the estimated effect lies between 4.1% or 7.3%, depending on the control group. For the time-effects model in the full sample the estimated effect is around 1.5% to 2.4%, while for the difference-in-differences model with the unscaled (two-period) sample, the estimated effect is between 1.8% and 2.3%. The likely reason for the different magnitudes here is that the full sample difference-in-differences averages over 1992 and 1997 to determine the pre-treatment period, whereas the latter two use 1997 only. The time-effects model for both county and state data suggests that sales in 1992 in Texas were relatively small.

The effects measured at county and state levels need not be the same: given our data transformations, the estimated state-level response essentially takes an average of the log of the sum of county sales, while the average county response is based on the average of the sum of the logs of county sales. The state level estimates thus give greater weight to

[31] Wyoming, Louisiana, North Dakota, New Mexico and Oklahoma match along the oil production dimension, the first of these three plus Arkansas, Arizona, Alabama and Nebraska match along the house-price dimension, and Arkansas, Louisiana, New Mexico and Oklahoma are border states.

high per capita spending counties than the county-level estimates. Only if spending shares are constant and equal across counties, or if all counties respond identically to the change, would we expect the overall state effect to equal the average county effect, *ceteris paribus*. Nonetheless, we do not expect large difference in estimates across the two samples. In this context, note the similarity of the responses of the average Texas county to the state-wide averages, especially for the difference-in-differences model, and for the year 2002 in the time effects model. Keep in mind that the county data do not factor in the years immediately following the amendment, which exhibit large effects in the state data.

When we restrict β to be zero, α is an estimate of the *average* county-wide treatment effect, which can mask variation in the effect across counties. Such variation may be important. For example, according to the American Community Survey of the Census, the percentage of households with home equity loans in Texas across fifteen selected counties in 2002 ranged from 0.93% to 11.8%, with a mean of 6.7% and standard deviation of 3.2%. These figures suggest that the extent to which credit constraints bind might vary across counties. Panel B of the table reports our estimates of α and β when we allow heterogeneous treatment effects along two dimensions for model (2).

In the first case, we set $z_{it} = ln(y_{it})$, the log of county-wide real per capita income. Low-income households are more likely to suffer from asymmetric information problems, less able to borrow on non-housing collateral or finance spending out of current wealth, and more impatient than high-income households, suggesting that borrowing constraints are more likely to bind.[32] To the extent these factors show up at the county level, relaxing home equity lending constraints will bring about a relatively large response from lower-income counties. The coefficient estimates in the table are consistent with this prediction,

[32]Low income households are likely to have suffered temporary negative income shocks, increasing their need to borrow and causing them to respond more than high-income individuals to enhanced credit availability. Income aggregated to the county-level, however, is necessarily more persistent than income at the individual level and thus less indicative of temporary income shocks. In general, the role of income and measures of wealth in explaining spending responses to relaxed credit constraints will differ in important ways across levels of aggregation.

are robust across models, and are statistically significant. A 10% increase in county-level per capita income reduces the treatment effect by about 1.2% (i.e., 120 basis points), regardless of sample and control. The table also reports the the implications of the estimates for the treatment effects at the 10^{th} and 90^{th} percentiles of the distribution of y_{it} across Texas in 1997. A Texas county in the lower 10% of the per capita income distribution is predicted to increase spending by between 6.2% and 11.2% relative to its counterfactual; in a county in the upper 10%, spending is constant or rises by only between 2% and 5%.[33]

Second, the credit-constraint hypothesis implies a positive relationship between the spending effect of the amendment and the available home equity in the county. To proxy for the amount of home equity, we set z_{it} to be 1 for counties in which real median housing prices in 1990 (the latest year prior to the amendment for which county-level house price data are available) exceeded the Texas state median house price in that year, and zero otherwise.[34] In this case, α measures the treatment effect in counties with relatively low borrowing potential, while β measures the increase in the effect for counties with high borrowing potential. Our findings suggest that high-potential counties have relative effects that are over 3 percentage points higher than low-potential counties.

We perform one additional exercise to examine the robustness of our regression results for the county data. As with the state data, we run placebo tests by re-estimating the models, pretending that the counties in each state in turn have been treated by a similar event, in a fashion similar to, for example, Anderson and Meyer (2000, pp. 96-100).[35] In general, for the constant effects case, the estimated Texas treatment effect falls in the middle of the placebo distribution. On the other hand, when we look at the placebo distributions across our estimates of the low-income and high-house-price counties, where we find stronger effects,

[33] Sales spillovers across county lines within Texas are likely to attenuate these effects if, for example, households in low-income counties shop extensively in high-income counties. Such spillovers will be much less likely, however, to affect state-wide estimates.

[34] Clearly, this proxy is imperfect, but it will be reasonable if variation in loan-to-value ratios across counties is small.

[35] Whereas the placebo tests using the synthetic control methods are systematic, here they are *ad hoc* in that we make no attempt to determine appropriate controls for each 'treated' state.

Texas falls in the upper 30%. We can't therefore rule out the possibility that our county-level effects are due to chance. However, we attribute a large measure of this uncertainty to the inability of the county data to capture the potentially large annual spending responses in 1999, 2000 and 2001.

4 Direct effects on borrowing and spending

We have stressed that estimate reduced form, aggregate responses. These include the direct effects on spending from households that incurred new collateralized debt after the amendment, spending financed by a decline in precautionary saving of households that no longer perceived future constraints, multiplier effects, and general equilibrium effects on goods and asset prices. We cannot separately identify these effects with the aggregate data. In this section, we attempt to disentangle the direct from indirect effects by estimating the response of home equity borrowing itself, using household level data from the American Housing Survey. Ideally, we would use household spending data to estimate direct spending effects, as in Leth-Petersen (2010); however, reliable data of this sort are hard to come by. An added benefit of looking directly at how home equity responds to the Texas law is that doing so can help confirm our results based on the retail sales data. Independent evidence that home equity borrowing *per se* rose in Texas after the amendment strengthens our identification and interpretation of the spending effects we find in the previous section.

What was the potential for new home equity lending in the state? Contemporary accounts estimated home equity there during the 1990's to have been around \$120 billion to \$200 billion, with the potential for collateralized lending estimated in the range of \$4 billion to \$10 billion annually.[36] According to Census data (Bennefield 2003), the total nominal value of specified owner-occupied housing units in Texas in 2000 was \$318 billion, so these values

[36] Skelton (1997, p. 4). See also the Texas State Comptroller's Office Special Financial Report on Home Equity Lending, April 1996.

seem reasonable. On a per capita basis (using Texas state-wide population in 1997 of 19.7 million), this projected range for new home equity loans was between $200 to $510 in nominal terms and $124 to $315 in constant CPI dollars.

There is some evidence at the state level that the amendment did indeed have a direct effect on home equity borrowing. According to American Housing Survey Reports (as noted by the Texas Comptroller of Public Accounts 2003), the number of Texans having home equity loans rose from 2.5% of all homeowners in 1997 to 4.5% in 1999, falling just below the US average of around 5%. By 2001, the percentage reached 6.4%, surpassing the national average. Regarding the value of loans, Table 3 reports calendar-year estimates of the flow of second-lien home equity loans and the total dollar value of these loans, from annual surveys undertaken by Texas's Office of Consumer Credit Commissioner since passage of the home equity law. The number of new second-lien loans in the state rose by 73% from 1998 to 1999 (45,549 to 78,843), while the dollar value of such loans rose from almost $900 million to $2.9 billion. These numbers imply that the average loan value rose from $19,718 to $37,362.[37] A separate survey from the Texas Comptroller (2003) reports average loans of $36,750 from 1998 to 2000 and more than $47,000 in 2001 and 2002. More precise aggregate data on home equity lending at the county or state level are not available.

We can, however, use household-level data to estimate the direct response of the law on home equity loans. The data we use are the from the American Housing Survey (AHS) Metropolitan Microdata Files. The data set contains detailed housing-related information at the household level for selected standard metropolitan statistical areas (SMSAs) in selected years. We rely on the files for eight SMSAs (Anaheim/Santa Ana, Buffalo, Milwaukee, Phoenix, San Bernardino/Riverside, San Diego, Dallas, and Fort Worth) that were surveyed in both 1994 and 2002. The data set has observations on whether a household obtained a second-lien home equity mortgage, the dollar amount of a such a mortgage at acquisition, and

[37] We can make no claims about the precision of these data. The survey reports raw values only from banks who respond, and makes no attempts to project values for the state as whole. Thus, these values are best thought of as rough lower limits to the state-wide flow of new home equity loans each year.

the year the mortgage was obtained. We are therefore able to compare home equity borrowing of a treated group (households in Dallas and Fort Worth) with a control group (households in the other six SMSA's) using the same difference-in-differences regression methods described above for the aggregate sales data.

The dependent variable in our household level regression is the log of the real value of (second-lien) home equity loans. The explanatory variables include a binary variable indicating whether the household resides in Texas, and an interaction term computed as the product of this binary variable and another set to 0 if the home equity loan is obtained prior to 1998 and 1 if obtained in 1998 or later. As above, we interpret the coefficient on the interaction term as an estimate of the percentage change in the dependent variable (here, home equity borrowing by Texas households) from before the amendment to after, relative to the change in such borrowing outside of Texas.[38] For control variables we include, in addition to SMSA-specific time trends, real household income, real house value, numbers of persons living in the household, and the age, sex, marital status and education of the household head. We account for unobserved metropolitan area fixed effects, and compute standard errors that are heteroscedasticity-consistent and that account for intra-metro (cluster) correlation. The sample includes only those households reporting non-zero home equity loans, and for which the age of the head of household is between 25 and 65 to capture established households before retirement (our results were not sensitive to this restriction). Of the 57,540 total observations, we are left with 2373 observations in our regression sample, of which 393 are in Texas.

We estimate that the average Texas household that took out a home equity loan increased such borrowing, in real terms, by between 25% and 34%, relative to non-Texans, depending on specification and controls. The robust standard errors are small and imply that the

[38]Besides the level of aggregation, an important difference with this regression vis à vis the aggregate regressions is that here we do not have a panel data structure; we can think of the treated and non-treated groups as coming from repeated random samples. This difference does not affect the interpretation of the coefficients of interest (Wooldridge 2002, pp. 128-30).

responses are not due to sampling error. The magnitude of this effect on actual borrowing is at least two times that found by Gross and Souleles (2002, p. 163) – they estimate a 13% increase in credit card debt for individuals subject to an increase in their credit lines. If we assume that the estimated effect for Dallas and Fort Worth is representative of the average county in Texas, we can use our estimates on the borrowing response to come up with estimates of the direct effect of the amendment on *spending*.

Table 4 contains the results of this simulation exercise. Panel A shows the implications of our estimates for direct borrowing and spending, while Panel B relates these results to the treatment estimates from the sales data. We consider the upper and lower bounds of our direct estimates, and two reasonable values for the overall treatment effect. Consider the first column of the table, which assumes the larger direct borrowing estimate of 34%. Prior to 1998, the average real value of a home equity loan in our household-level sample was $16,877. Our estimate of 34% log growth rate thus implies a real increase of $6,834 in borrowing after 1997, conditional on a household taking out a home equity loan. In 2002, 6% of Texas homeowners were reported to have a second home loan while the homeownership rate was 64%, so the *per capita* real increase in home equity borrowing was $262 ($0.06 \times 0.64 \times 6,834$, assuming that the average size of households did not differ systematically across those with and without second mortgages), or $5.16 billion in real dollar terms. Note that these values fall within the projected ranges above.

We should not expect all of the increase in second mortgages to go to non-housing consumption. The survey evidence of Brady, Canner and Maki (2000, pp. 446) suggests that 18% of US home equity extractions in 1998 and 1999 were used for consumer expenditures.[39] Using this as a reasonable estimate of how much non-housing expenditures rise with home equity funds, and further assuming that retail sales account for half of overall consumer

[39] This value is consistent with the findings of Mian and Sufi (forthcoming, p. 19) who produce econometric evidence suggesting that housing equity extractions are not used primarily to move up to a new home, buy investment properties or financial assets, or pay down credit card balances, so that consumer expenditures rise.

spending, the *direct* total consumption effect of the Texas amendment from home equity borrowing is estimated to be \$47 per person, in real terms, with a direct effect on retail sales of \$24.

Average annual real sales per capita in Texas counties over the period 1992 to 1997 were \$5,500. Thus, if the direct spending effect were the only response, per capita sales would have risen by 0.43%. Using 2.0% from the previous section as a reasonable and conservative estimate of the aggregate sales treatment effect, we infer that per capita spending rose by \$110 with the amendment, or 42% of the \$262 projected increase in home equity loans. Thus, for this set of assumptions, 21% of our estimated \$110 aggregate sales increase occurs because of the direct response of borrowing, with the remainder coming from the indirect multiplier and general equilibrium effects noted above. This percentage is an upper bound; the value falls to 10% if the direct borrowing response is 25% (second column) and the treatment effect is 3%.

Our results on the direct aggregate spending effects match up nicely to those of Leth-Petersen (2010, p. 1101), who uses his estimates at the household level to impute the direct spending response in aggregate (in his case, to the national level). His estimated aggregate expenditure effect of 0.3% for Denmark's credit reforms falls exactly at the lower bound of the range we report in the table.[40] We are thus able to reconcile the differences between our aggregate level estimates and his findings at the micro level as likely being due to the general equilibrium effects we pick up at the county and state levels. It is encouraging that our results in Texas are similar to those in Denmark for essentially the same policy intervention, despite presumably large differences in population characteristics.

There is at least one reason, however, to believe that our direct estimates under-predict the effect on spending of relaxing borrowing constraints that bind. Our estimated direct effects from the AHS exclude the spending responses of households that did not actually

[40]Recall that his estimates are for total consumption, rather than retail sales. To the extent that the sales-to-consumption ratio remains constant, we can properly compare our implied growth rates with his.

borrow on home equity, but who, knowing that the law reduced *future* borrowing constraints, spent more of their disposable income by reducing precautionary savings. Presumably, such effects are captured in Leth-Petersen's estimates. Thus, relaxing credit constraints in Texas likely had a larger effect on spending than in Denmark, but probably not by much.

5 Conclusion

Our study implies that credit constraints matter for aggregate retail spending in Texas. Spending in the average Texas county rose by about 2% from 1997, the year the Texas amendment passed, to 2002 (and by a bit more when spending in 1992 is factored into the analysis). Spending rose even more in low-income Texas counties – in the 6 to 8% range – and in counties with high house prices prior to the amendment. The effects are larger at the state level for annual sales in the years immediately following the 1997 amendment, where they reach 6 or 7% after the clarifying amendments in 1999, but the effects do not appear permanent as they decline in 2002. A look at household level data on home equity loans confirms the significance of these estimates by showing that the Texas law caused a jump in home equity borrowing in the state. We use these data to decompose our aggregate effects into the response of spending directly due to borrowing, and the indirect responses due to general equilibrium, reduced form effects. Our estimates are consistent with, though perhaps slightly larger than, those found by Leth-Petersen (2010) from a similar natural experiment in Denmark.

These findings are important because they provide another set of observations on how people respond to credit-availability shocks, and thus the relevance of credit constraints for economic decisions and policy. Our results also provide further evidence that the link between house prices and non-housing expenditures is most likely the result of 'collateral effects' rather than 'wealth effects.' The Texas experiment did nothing directly for homeowners in terms of their wealth. But for credit-constrained homeowners, the home equity amendment was roughly tantamount to having their purchasing power for current spending rise by the difference between their current mortgage debt and (80% of) the market value of their home. The spending responses to Texas's 'natural experiment' are thus most likely caused by newly available collateral, not newly perceived wealth.

Appendix

THE 1997 TEXAS CONSTITUTIONAL AMENDMENT

Abridged version of Article XVI, Section 50 of the Texas Constitution, with the salient amendments from Proposition 8 in 1997 in italics. We do not show 15 additional stipulations [(C) through (Q)] of sub-section (6) that aimed to protect consumers, or additional details on reverse mortgages in sub-section (7).

Article XVI. General Provisions

Sec. 50. Homestead; protection from forced sale; mortgages, trust deeds and liens.

(a) 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:

1. the purchase money thereof, or a part of such purchase money;
2. the taxes due thereon;
3. an owelty of partition imposed against the entirety of the property by a court order or by a written agreement of the parties to the partition, including a debt of one spouse in favor of the other spouse resulting from a division or an award of a family homestead in a divorce proceeding;
4. the refinance of a lien against a homestead, including a federal tax lien resulting from the tax debt of both spouses, if the homestead is a family homestead, or from the tax debt of the owner;
5. work and material used in constructing new improvements thereon, if contracted for in writing, or work and material used to repair or renovate existing improvements thereon ...
6. *an extension of credit that:*
    (a) *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;*
    (b) *is of a principal amount that when added to the aggregate total of the outstanding principal balances of all other indebtedness secured by valid encumbrances of record against the homestead does not exceed 80 percent of the fair market value of the homestead on the date the extension of credit is made; ...*
7. *a reverse mortgage;...*

DATA SOURCES

County data

- Retail sales: US Census Bureau, Economic Census of Retail Trade, Final Reports of Geographic Area Series for 1992, 1997 and 2002.

  census.gov/prod/1/bus/retail/92area/92ret.html

  census.gov/prod/www/abs/ec1997retail.html

  census.gov/prod/www/abs/ec2002retail.html

- Personal income: Bureau of Economic Analysis (NAICS basis).

  censtats.census.gov/

- Unemployment rate: Bureau of Labor Statistics, Local Area Unemployment Statistics

  bls.gov/lau/data.htm

- Median house prices: Census Bureau, Decennial Census of Housing

  census.gov/hhes/www/housing/census/censushousing.html

State data

- State sales tax revenues: General sales and gross receipts, Annual Survey of State Government Tax Collections

  census.gov/govs/statetax/historical_data.html,

- State sales tax rates: Sales Tax Collections and Rates (various issues)

  taxfoundation.org/publications/dates/2000.html

  State Sales, Gasoline, Cigarette and Alcohol Taxes by State, 2000-2010

  taxfoundation.org/taxdata/show/245.html.

- Personal disposable income: Bureau of Economic Analysis

  bea.gov/iTable/iTable.cfm?reqid=70&step=1&isuri=1&acrdn=4

- Oil prices: West Texas Intermediate, St. Louis Federal Reserve Data Base

  research.stlouisfed.org/fred2/series/OILPRICE

- Mortgage rate and house prices: Monthly Survey of Rates and Terms on Conventional Single-Family Non farm mortgage loans, Federal Housing Finance Agency, Historical Summary Tables, Table 15

  fhfa.gov/Default.aspx?Page=252

Household data

American Housing Survey Metropolitan Microdata Files – 1974-1983 and 1984-2004.
huduser.org/portal/datasets/ahs/ahs_cd.html

- Second-lien home equity loans: AMMRT2
- Year second-lien home equity loan obtained: YRMOR2
- Household income: ZINC2
- House value: VALUE
- Household size: PER
- Age, sex, marital status, and years of education of head of household: HHAGE, HHSEX, HHMAR, HHGRAD

OTHER

- All nominal dollar values are deflated using the CPI for all urban households
- Population values used for per capita computations:

  census.gov/popest/archives/2000s/vintage_2001/CO-EST2001-12/

  census.gov/popest/states/NST-ann-est.html
- Aggregate home equity loans in Texas: Home equity loan reports, Office of Consumer Credit Commissioner, Texas Finance Commission.

  fc.state.tx.us/homeinfo/herptcy05.pdf

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Table 1: STATE-LEVEL TREATMENT EFFECTS

	Control: all-states			Control: reduced-states		
	FE	FD	SYNTH	FE	FD	SYNTH
Panel A: Time-effects model, equation (1)						
α_{92}	0.002	-0.010	0.008	-0.016	-0.018	0.001
	(0.012)	(0.008)		(0.012)	(0.011)	
α_{93}	0.008	-0.008	0.008	-0.009	-0.015	-0.003
	(0.012)	(0.008)		(0.010)	(0.009)	
α_{94}	0.024	0.016	0.008	0.010	0.011	0.005
	(0.014)	(0.010)		(0.009)	(0.008)	
α_{95}	0.021	0.013	0.008	0.010	0.009	-0.002
	(0.012)	(0.008)		(0.008)	(0.006)	
α_{96}	0.014	0.005	0.008	0.006	0.002	-0.003
	(0.008)	(0.004)		(0.005)	(0.003)	
α_{97}	–	–	0.011	–	–	0.003
α_{98}	0.019	0.032	0.045	0.030	0.036	0.034
	(0.011)	(0.016)		(0.015)	(0.017)	
α_{99}	0.042	0.052	0.048	0.055	0.056	0.036
	(0.022)	(0.026)		(0.027)	(0.027)	
α_{00}	0.047	0.054	0.044	0.065	0.059	0.028
	(0.027)	(0.028)		(0.033)	(0.030)	
α_{01}	0.056	0.065	0.052	0.076	0.069	0.016
	(0.031)	(0.033)		(0.038)	(0.035)	
α_{02}	0.026	0.041	0.028	0.046	0.045	-0.012
	(0.019)	(0.023)		(0.026)	(0.025)	
Panel B: Difference-in-differences, equation (2)						
α	0.022	0.027	0.043	0.032	0.028	0.021
	(0.012)	(0.013)	–	(0.016)	(0.014)	–

Notes: Treatment effects from equations (1) and (2) for fixed effects (FE) and first-difference (FD) estimation, and from the synthetic control counterfactual in equation (3). The standard errors in parentheses are heteroscedasticity and autocorrelation consistent.

Table 2: County-level treatment effects

	Sample: 1992,1997,2002			Sample: 1997,2002		
Control group	all-states	reduced-states	matched-states	all-states	reduced-states	matched-states
Panel A: Constant effects ($\beta = 0$)						
Time-effects model						
α_{92}	-0.058	-0.059	-0.065	–	–	–
	(0.028)	(0.028)	(0.030)	–	–	–
α_{02}	0.015	0.016	0.024	–	–	–
	(0.008)	(0.009)	(0.013)	–	–	–
Difference-in-differences model						
α	0.041	0.048	0.073	0.023	0.023	0.018
	(0.021)	(0.024)	(0.043)	(0.011)	(0.011)	(0.010)
Panel B: Heterogenous effects ($\beta \neq 0$)						
z=log per capita real income						
α	0.339	0.346	0.371	0.322	0.322	0.316
	(0.161)	(0.167)	(0.179)	(0.153)	(0.155)	(0.150)
β	-0.118	-0.119	-0.119	-0.119	-0.119	-0.118
	(0.056)	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)
$\alpha + \beta z_{10}$	0.082	0.087	0.112	0.063	0.063	0.059
$\alpha + \beta z_{90}$	0.021	0.025	0.050	0.001	0.001	-0.002
z = house price binary variable						
α	0.026	0.033	0.058	0.007	0.007	0.003
	(0.015)	(0.017)	(0.037)	(0.004)	(0.004)	(0.006)
β	0.031	0.031	0.032	0.032	0.032	0.031
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)

See notes to Table 1. Results are presented for FD estimator only. Results in Panel B for model (2) only.

Table 3: Second-lien home equity loans in Texas

	Loans issued	Total value of loans
1998	45,549	898,129,428
1999	78,843	2,945,768,671
2000	30,072	906,907,719
2001	25,798	794,067,427
2002	29,287	1,324,552,412

Notes: The values are in nominal terms, and sum the reported categories “loans made,” “loans brokered,” and “loans receivable.” Source: Office of Consumer Credit Commissioner, Home Equity Lending Reports, 2001-2005. Some values from the 2001 report, not used here, contradict those of the later reports.

Table 4: Estimating direct spending effects

	—AHS estimate—	
	34%	25%
Panel A: Direct effects on borrowing and spending		
New loans	\$6,834	\$4,794
New loans per capita	\$262	\$184
New sales per capita	\$24	\$17
% New sales per capita	0.0043	0.0030
Panel B: Direct effects relative to treatment effects		
$\alpha = 2\%$		
New sales per capita from treatment estimates	\$110	\$110
Treatment sales relative to new loans	0.42	0.60
Direct sales relative to treatment sales	0.21	0.15
$\alpha = 3\%$		
New sales per capita from treatment estimates	\$165	\$165
Treatment sales relative to new loans	0.63	0.90
Direct sales relative to treatment sales	0.14	0.10

Computations described in the text. Assumptions: prior average home equity loans = \$16,877, prior average real per capita sales in Texas = \$5,500, % home equity loans going to consumption = 0.18, % consumption going to retail sales = 50%.

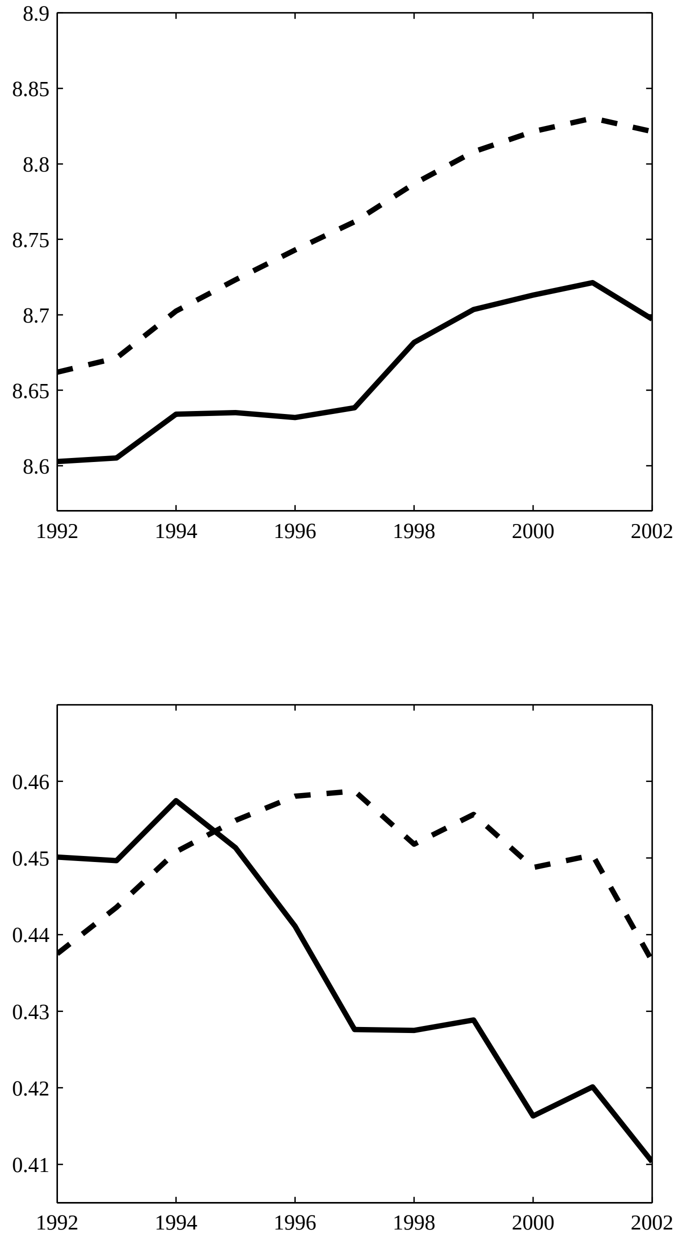


Figure 1: Log real per capita retail sales (upper panel) and sales relative to income (lower panel); solid curve: Texas; dashed curve: average of 44 other states in the state sample.

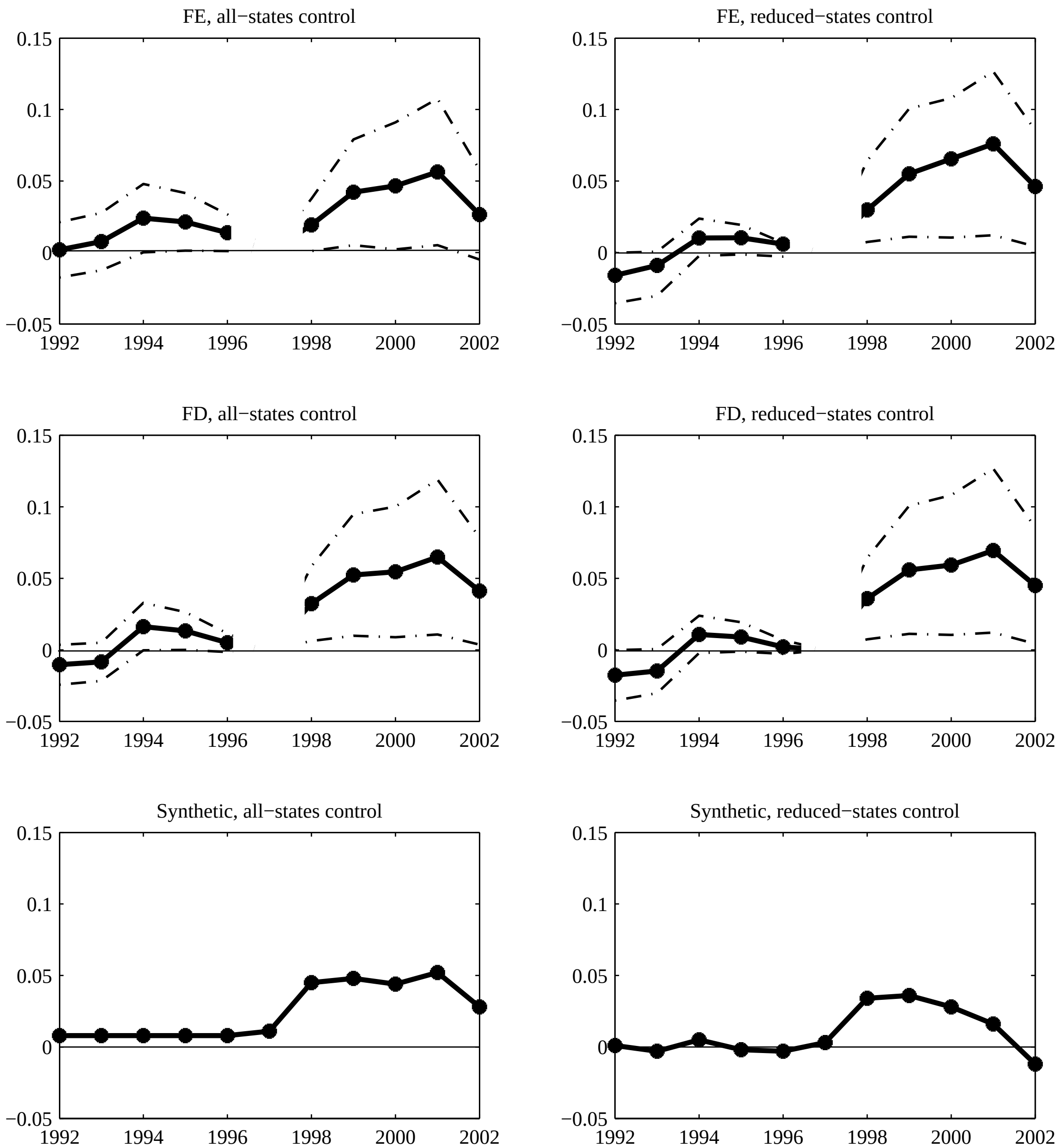
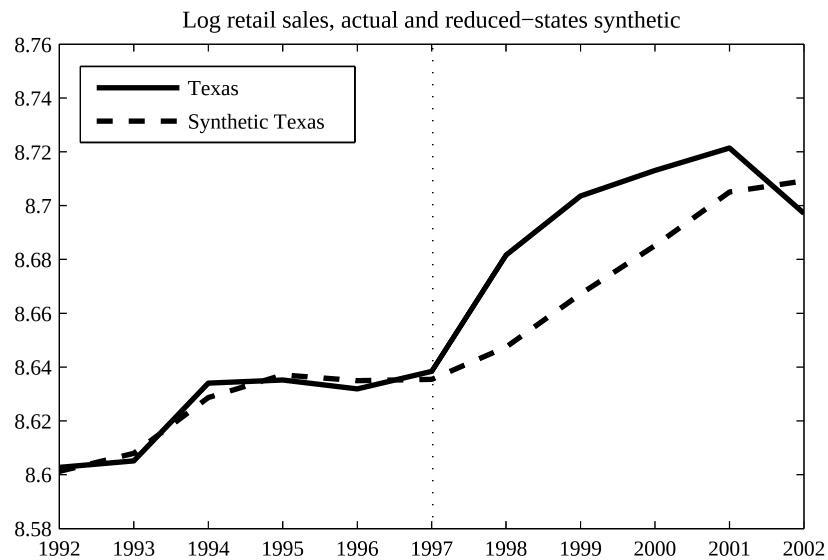


Figure 2: Estimated state treatment effects from Table (1) with 90% confidence intervals (1.64 standard error bands). 1997 is the base year and is omitted.



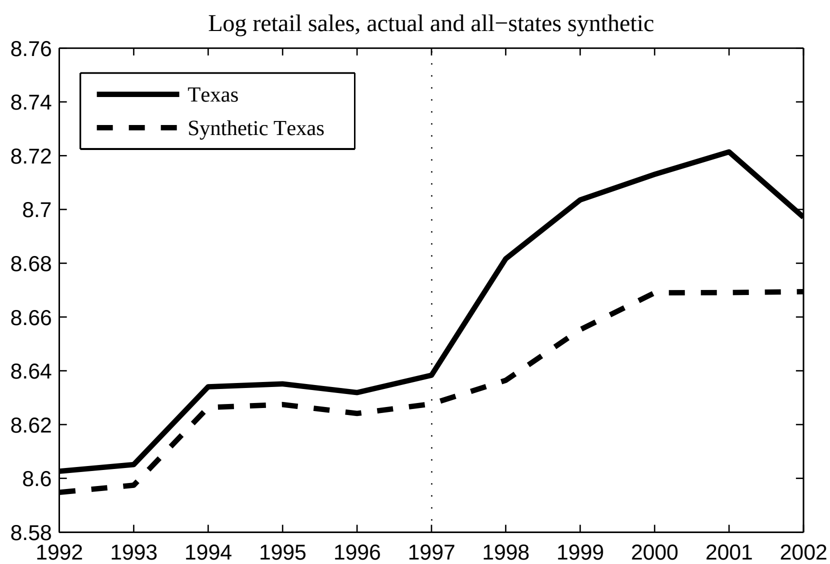


Figure 3: Log of actual Texas real per capita sales (solid) and synthetic Texas sales (dashed).

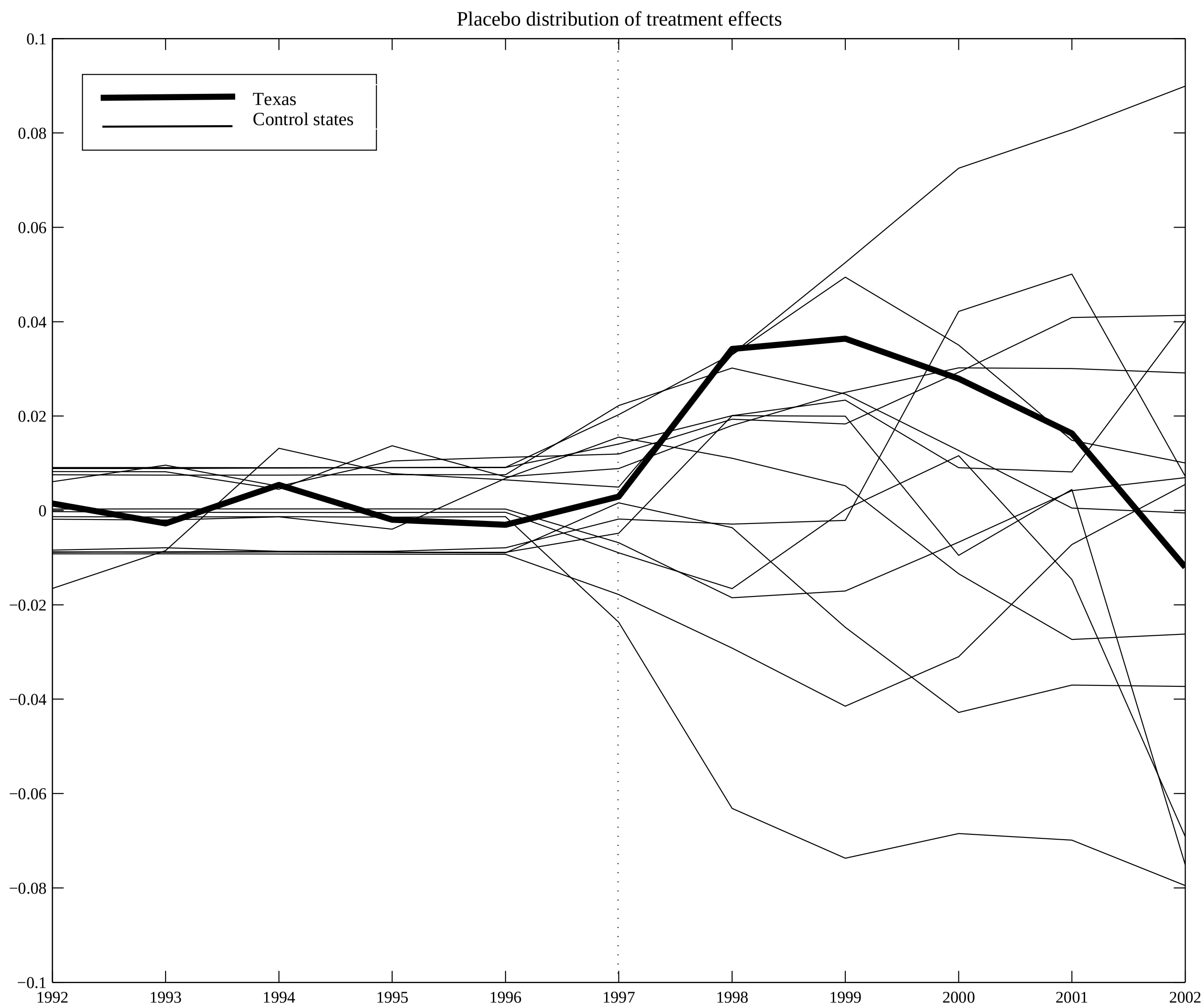


Figure 4: Placebo distribution of treatment effects for reduced donor pool.