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Working Paper 1810 **August 2018 (Revised February 2023)**

Research Department

https://doi.org/10.24149/wp1810r3

Labor Market Effect of Credit Constraints: Evidence from a Natural Experiment*

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Revised: January 2023

Abstract

We exploit the 1997 and 2003 constitutional amendments in Texas—allowing home equity loans and lines of credit for non-housing purposes—as natural experiments to estimate the effect of easier credit access on the labor market. Using state-level as well as micro data and the synthetic control approach, we find that easier access to housing credit led to a 1.2 percentage point average decline in the labor force participation rate between 1997 and 2007. We show that our findings are remarkably robust to improved synthetic control methods based on insights from machine-learning. We also find that declines in the labor force participation rate were larger among females, prime age individuals, the college-educated, and homeowners. Our research shows that negative labor market effects of easier credit access should be an important factor when assessing its stimulative impact on overall growth.

Keywords: Credit Constraints and Labor Supply, Synthetic Control with Machine Learning

JEL Codes: J21, R23, E24, E65

*We thank Carlos Zarazaga, Mike Weiss, Albert Zevelev, Daniel Ringo, Aimee Chin, Seema Jayachandran, Fan Wang, Mehtabul Azam, seminar participants at the Oklahoma State University, conference participants at the STATA-Texas Empirical Micro Conference (Waco, TX), Federal Reserve System Applied Microeconomics Conference (Boston, MA), NTA Annual Conference on Taxation (New Orleans, LA), and the Western Economic Association International (Vancouver, Canada), for helpful comments. We thank Nick Doudchenko and Guido Imbens for sharing the code for synthetic control model estimation with elastic net penalty. The views expressed here are those of the authors and do not necessarily reflect those of the Federal Reserve Bank of Dallas or the Federal Reserve System.

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1. Introduction

With over a quarter of American wealth and more than two thirds of household debt tied to housing, house price changes and access to mortgage debt secured by the housing collateral play a central role in the transmission of monetary policy. Given that housing wealth is illiquid and a vast majority of American households face binding credit constraints, improving access to the housing collateral remains an important goal for public policy. Using instrumented house price changes in combination with proxies for collateral constraints, a large body of previous research has found that the collateral channel, rather than the housing wealth effect, is the primary mechanism through which house price changes affect consumer spending (Cooper, 2013; Aladangady, 2017; Cloyne, Ilzetzki, and Kleven, 2019).

However, identifying the housing collateral effect using local house price changes poses formidable challenges, as variation in collateral values happens concomitantly with changes in housing wealth. Just a handful of papers overcomes the identification challenge by using plausibly exogenous policy variations in access to housing collateral as natural experiments (Leth-Peterson, 2010, Abdallah and Lastrapes, 2012; De Fusco, 2018). While estimates of average effects vary, the general conclusion of this line of research is that households facing binding credit constraints exhibit a strong borrowing response when housing collateral constraints are relaxed, although not all of the additional borrowing is used to finance current consumption.[1]

Any response on margins other than consumption has important implications for the effectiveness of monetary policy to influence aggregate demand, at least in the short term. As noted in Disney and Gathergood (2018), it also raises the possibility that at least a part of the borrowing

[1] De Fusco (2018) noted that “at least some fraction of the borrowed money was used to fund current expenditures”. More recent research on the effect of exogenous income changes finds that an additional dollar of unearned income increases consumption by 60 cents, reduces labor earnings by 50 cents, and lowers taxes by 10 cents (Golosov et. al., 2021).

is spent to fund leisure rather than smooth consumption.[2] With the literature almost exclusively focused on consumption responses, there has been no formal investigation of the direct effects of the housing collateral on labor supply using exogenous policy variation in collateral constraints that leave housing wealth unchanged.[3]

We fill this gap and provide the first direct causal evidence of the effects of housing collateral constraints on the labor market by exploiting the 1997 and 2003 constitutional amendments in Texas as natural experiments. While home equity borrowing had all along been available to homeowners in all other states, they remained off limits for non-housing purposes in Texas, until the 1997 amendment for the first time opened up access to closed-end home equity loans (HEL) and cash-out refinancing. Subsequently, the 2003 amendment legalized home equity lines of credit (HELOC).[4] In estimating labor market effects of the Texas amendments, we focus on the labor force participation rate (LFPR), and in addition to estimating standard difference-in-differences (DID) specifications and employing the regular synthetic control approach, we also present applications of newly developed synthetic control methods based on machine learning.[5]

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[2] Using a three-period setting with collateral constraints, in Appendix B we show that while easier access to home equity could lower labor supply in the first period, overall effects on labor supply are far from clear, as theoretical effects turn ambiguous in the second period.

[3] A more distinct stream of research has explored the relationship between the broader housing market and labor supply, generally finding negative wealth effects of house price growth, consistent with leisure being a normal good (Disney and Gathergood, 2013; Milosch, 2014; Atalay, Barrett, & Edwards, 2016; Fu, Liao, & Zhang, 2016; Bottazzi, Trucchi, & Wakefield, 2017; Zhao and Burge, 2017; Li et. al. 2020). But a consensus on the effect of house price growth on labor supply remains elusive (Yoshikawa and Ohtake, 1989; Johnson, 2014; He, 2015).

[4] By focusing on labor market effects, the paper complements a small set of recent papers that exploited the 1997 Texas amendment as a source of exogenous shock for outcomes other than labor supply. Most notably, Abdallah and Lastrapes (2012) provided compelling evidence that increased access to home equity borrowing spurred consumer spending. More recently, Zevelev (2021) showed that by removing restrictions on home equity borrowing, the 1997 Texas amendment contributed to a 3 to 5 percent increase in house prices over the 6 years following the law change. Stolper (2014) found that the 2003 amendment led to gains in access to higher education financed by home equity borrowing. However, no paper examined labor market effects.

[5] We focus on LFPR rather than hours of work precisely for two reasons. First, the Bureau of Labor Statistics (BLS) spends considerable efforts in accurately measuring LFPR at the state-level through its Local Area Unemployment Statistics (LAUS) program, while such reliable measures of hours of work at the state-level are not available. Secondly, labor supply is known to be significantly more elastic on the participation rather than the hours margin (Heckman, 1993).

Plotting weighted-averages of state-level LFPR using widely available BLS data, Figure 1 provides a first glimpse of the LFPR decline in Texas relative to the rest of U.S. after home equity access became available in 1998. While informative, such simple comparisons between Texas and the U.S. could conflate the impact of home equity access in Texas with the effects of other macroeconomic shocks and policies that may have changed concomitantly and affected Texas differently than other states. For example, the period surrounding the Texas amendments saw sharp swings in oil prices (Appendix Figure A1), and it is well-known that oil-price shocks affect Texas differently than most other states (Murphy, Plante & Yücel, 2015). Furthermore, Texas could have reacted differently to welfare policy changes and the Earned Income Tax Credit (EITC) expansions implemented in the 1990s. We adopt a careful and comprehensive approach to address these concerns.

Using aggregate state-level as well as micro data, we find that, by opening the home equity lending market to Texas' homeowners, the 1997 and 2003 amendments led to persistent declines in the LFPR between 1997 and 2007. We first show that conventional difference-in-differences specifications comparing the LFPR in Texas with other states before and after the law changes yield negative effects on the LFPR but may be subject to biases due to pre-existing differential trends in the LFPR in Texas vis-à-vis the nation. We, therefore, employ synthetic control methods that account for the potential violation of the common trend assumption. We proceed by optimally weighting comparison states to construct a synthetic control that has pre-treatment LFPR trends almost identical to those in Texas (Abadie and Gardeazabal, 2003; Abadie, Diamond, & Hainmueller, 2010; Abadie, Diamond, & Hainmueller, 2015).

While the standard synthetic control method remains overwhelmingly popular in settings with just one treated unit, recent research has proposed important refinements that relax some of the underlying restrictions in the traditional method and, using machine learning techniques, enhance its suitability in situations with limited number of control units and a small number of pre-treatment periods. We employ two such approaches to demonstrate the robustness of our baseline synthetic control estimates: (1) the balancing method with elastic net penalty proposed in Doudchenko and Imbens (2016) and (2) the matrix completion approach suggested in Athey et al. (2021).

Our preferred estimates suggest that access to home equity borrowing led to a 1.2 percentage point average decline in LFPR over 10 years between 1997 to 2007. The LFPR declined 0.8 percentage on average between 1997 and 2003, when just HELs and cashout refinancing were available, but the average treatment effect strengthened to 1.8 percentage points after HELOCs were also allowed in 2003.[6] We explore treatment effect heterogeneity across demographic groups using basic monthly CPS data and find that easier credit access led to relatively larger declines in LFPR of females, the prime-age population, the college-educated, and homeowners.

While the data does not allow us to precisely pin down all mechanisms, there are multiple potential channels through which collateral constraints can affect labor supply. First, by reducing uncertainty, easier collateral constraints should alleviate the need for precautionary saving (Agarwal and Qian, 2017) and precautionary labor supply (Basu and Bundick, 2017). Secondly, it is well-known that increased access to home equity leads to higher fertility, affecting labor supply of women of childbearing age (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014). Thirdly, the presence of credit constraints can also contribute to increased LFPR of married

[6] Our paper is also related to previous work that tested the standard life-cycle model's prediction that credit-constrained households can smooth consumption by increasing labor supply (Worswick, 1999; Bui and Ume, 2016; Rossi and Trucchi, 2016). A related but somewhat separate strand of the literature focused primarily on the labor supply effects of higher debt and found positive effects of mortgage debt commitments on labor supply, mainly involving married females (Fortin, 1995; Aldershof, Alessie, & Kapteyn, 1997; Del Boca and Lusardi, 2003; Bottazzi, 2004; Houdre, 2009; Maroto, 2011; Butricia and Karamcheva, 2013; Lusardi and Mitchell, 2017; Cao 2017). But the evidence remains far from conclusive, as other papers find contrasting results (Bernstein, 2015; Pizzinelli, 2017).

females through an added worker effect; therefore, relaxing these constraints should lower labor supply (Lundberg, 1985). Fourthly, home equity can be tapped to fund college enrollment, which also could lower labor supply of college age individuals. And finally, as found in Zevelev (2021), the ability to pledge the housing collateral led to higher house prices in Texas, which would have further amplified the collateral effects on labor supply.

Our estimates have important implications for countries or regions where a significant part of housing wealth is locked up in home equity that cannot be tapped, either due to regulations or because the financial markets aren't sufficiently developed to allow easy borrowing against the housing collateral. To be sure, providing households easier access to untapped home equity could boost consumer spending but may also lower the LFPR. Thus, our estimates shed light on the effect of financial frictions on the labor market, though a key limitation is that we are unable to pin down long-term welfare effects. While more leisure increases welfare, it also means less earnings and consumption and slower economic growth, which may offset some of the welfare gains from easier credit access.

The rest of the paper is organized as follows. Section 2 discusses the 1997 and 2003 amendments in Texas allowing home equity access. Section 3 describes the data. Econometric specifications and estimation results are discussed in section 4, and section 5 concludes.

2. The 1997 and 2003 Home Equity Amendments in Texas

Before 1998, the Texas constitution greatly restricted collateralized borrowing against home equity. While home buyers could use their home as collateral to obtain mortgage to finance the home purchase, subsequent home equity borrowing was severely limited. Aside from the mortgage to purchase the home, the Texas constitution allowed using the home as collateral primarily for just home improvement loans (Graham, 2007). Almost all other forms of home equity borrowing remained out of bounds for Texas homeowners.[7] For example, cash-out refinancing, a widely used form of home equity extraction in the rest of U.S., was not permitted. While refinancing, home equity could be used only to cover the cost of refinancing. Home equity loans through second mortgages or home equity line of credit remained off limits.

In November 1997, Texas' voters approved House Joint Resolution 31 (HJR 31), amending Section 50, Article XVI of the Texas constitution to allow home equity loans through second mortgages or cash-out refinancing but capping the borrowed amount to no more than 80 percent of a home's appraised value.[8] The amendment took effect on January 1, 1998. Although total borrowing against home equity was capped in Texas, anecdotal reports indicate that access to home equity loans and cash-out refinancing led to significant expansion of mortgage credit after the amendment became law.

While authorizing home equity borrowing for non-housing purposes, the 1997 amendment allowed only traditional closed-end home equity loans that must be repaid in "substantially equal successive periodic instalments", thus prohibiting HELOCs—revolving accounts with a maximum credit limit available for use at the borrower's discretion for a draw period of typically 10 years at a variable rate of interest. A HELOC typically involves interest-only payments on the credit accessed during the draw period; any outstanding balance must be paid off within a set repayment period after the draw period expires. The 2003 amendment for the first time authorized HELOCs

[7] Since 1995, in the event of divorce, jointly owned homes could be converted to full ownership through a home equity loan to pay off the joint owner's share of home equity. For more details on the provisions of the constitutional amendment, see Graham (2007), Abdallah and Lastrapes (2012), Kumar and Skelton (2013), Kumar (2018), and Zevelev (2021).

[8] HJR 31 was presented to voters as Proposition 8. In addition to the cap on the home equity lending Texas also has some other provisions to curb predatory lending as summarized in Graham (2007). Additionally, the Texas law allows only one home equity loan at a time and in case of refinancing, only one refinancing per year. The 1997 constitutional amendment also prohibited home equity loans with balloon payments, negative amortization, and pre-payment penalties. Further, HELOCS remained prohibited until 2003.

in Texas, subject to the 80 percent limit on Combined-Loan-to-Value (CLTV) ratio and other consumer protection limitations (Graham, 2007).

Before presenting the main results, it is important to examine whether and to what extent the twin amendments spurred home equity borrowing. Using American Housing Survey data on 8 SMSAs (with Dallas and Fort Worth representing Texas) in 1994 and 2002, Abdallah and Lastrapes (2012) estimated that per-capita borrowing through HELs increased by $263 in real terms after the amendment—from $110 in 1997 to $373 in 2002. We supplement their analysis by examining originations of new HELs (before vs. after 1997) and HELOCs (before vs. after 2003) using the New York Consumer Credit Panel (NYCCP). Additionally, we also analyze the impact of the 1997 law change on the origination of cash-out refinance loans in Texas using Residential Mortgage Servicing Database from Black Knight Financial Services (BKFS). Panel A of Table 1 reports DID estimates for the number of loans and Panel B for amount originated per-homeowner (both measured in logarithms). Both number of loans and total amount of originated loans saw notable increases in Texas after the law change relative to the rest of U.S.[9]

3. Data

Our baseline difference-in-differences and synthetic control estimates are based on state-level data from 1992–2007 on 50 states, spanning 6 years before and 10 years after the amendment that allowed home equity access in Texas. Starting with 1992 helps us avoid differential trends in Texas vs. other states due the 1980's recessions, the savings and loan crisis, and the 1991 recession.

[9] While conventional clustered standard errors indicate significance for all measures, Conley-Taber confidence intervals suggest that the effect on cash-out refinance loans and HELOCs after 2003 were significant. Our estimates are subject to some caveats and should be viewed as suggestive at best. Results using NYCCP data are based on loans originating between 1995 and 2000, which remained active after 1999. BKFS data covers two-thirds of all instalment-type loans, issued by the top-10 mortgage servicers.

We stop in 2007 because, after the Great Recession, the Texas economy followed a very different path from the national economy, once again due to large swings in oil prices. That leaves us with 1992-1997 for the pre-treatment period and 1998-2007 for the post-treatment period.

Our primary outcome variable is the LFPR. State-level data on the LFPR is from the Local Area Unemployment Statistics (LAUS) program of the Bureau of Labor Statistics (BLS).[10] We use average hourly earnings of manufacturing workers as the measure of hourly wages, also from the BLS. Both, the LFPR and wages, are available at monthly frequencies, which we average at the annual level to avoid highly volatile month-to-month movements in CPS data at the state level. The state-level average income tax rate is calculated as the ratio of state-level income tax receipts to state-level personal income, with data on both from the Bureau of Economic Analysis (BEA). We use annual averages of quarterly state-level data on house prices from the Federal Housing Finance Agency (FHFA). We then merge the state-level annual averages of demographic variables—age, race, sex, marital status, presence of children in the household, and education—calculated from monthly basic CPS data available from IPUMS-CPS (Flood, King, Ruggles, & Warren, 2015).

Table 2 presents summary statistics for key variables from the state-level data. Results using micro data to explore treatment effect heterogeneity are primarily based on annual averages by demographic groups constructed using basic monthly CPS files from the IPUMS-CPS. Since basic monthly CPS lacks information on homeownership and, more importantly, because composition of the sample may change in repeated cross-section data due to rising homeownership rates, we use panel data from the Panel Study of Income Dynamics (PSID) from 1992 to 2007 in

[10] We also test the robustness of our state-level estimates to use of county-level data and present results in the appendix.

specifications with individual fixed effects to examine differences in estimated effects for homeowners vs. renters.[11]

4. Econometric Specification and Estimation Results

4.1 Difference-in-Differences Specifications

Using state-level data to estimate the effect of the 1997 and 2003 amendments in Texas, our benchmark difference-in-differences (DID) specification with state and time-fixed effects is as follows:

$$Y_{st} = \beta^{HEL} D_s^{TX} \times D_t^{Post-HEL} + \beta^{HELOC} D_s^{TX} \times D_t^{Post-HELOC} + \boldsymbol{X}_{st}\gamma + \delta_t + \alpha_s + \eta_{st}, \qquad (1)$$

where Y_{st} is the primary outcome variable (LFPR), D_s^{TX} is a dummy variable for the treated state Texas, $D_t^{Post-HEL}$ is a dummy variable for the 1998–2003 period when only home equity loans (HEL) were allowed and HELOCs remained out of bounds, $D_s^{TX} \times D_t^{Post-HEL}$ is an indicator variable that equals 1 for the treated group (Texas) in the Post-HEL period from 1998 to 2003 and 0 otherwise. Allowing the effect of access to both HEL and HELOC to differ from that of just HEL, we additionally include the interaction $D_s^{TX} \times D_t^{Post-HELOC}$ to capture the effect in the post-HELOC period (2004–2007). α_s are state fixed effects; δ_t represents time effects; $\boldsymbol{X}_{st}$ is a vector of economic and demographic covariates that vary across states as well as over time, and η_{st} are random state-by-time effects. All states other than Texas serve as the control group. Coefficients on the policy variables, β^{HEL} and β^{HELOC}, are the DID estimates of the effects of access to just HEL and both HEL and HELOC, respectively.[12]

[11] We use PSID-CNEF data files available from https://cnef.ehe.osu.edu/data/ and supplement them with variables from the main PSID files available from https://psidonline.isr.umich.edu/. See Burkhauser et. al. (2001) for more information about PSID-CNEF.

[12] More specifically, β^{HEL} represents the DID effect for the period 1998-2003 relative to the pre-HEL period 1992-1997 while β^{HELOC}captures the effect for the post-HELOC period 2004-2007.

In this framework, the state fixed effects account for pre-existing differences in the LFPR between Texas and the rest of U.S, while the year effects control for purely time-varying differences due to macroeconomic shocks common to the state as well as to the nation. The DID identifying assumption is that, conditional on the fixed effects and covariates, state-by-time effects, η_{st} is random and uncorrelated with the policy variables ($D_s^{TX} \times D_t^{Post-HEL}$ and $D_s^{TX} \times D_t^{Post-HELOC}$). In other words, trends in Texas' LFPR must be parallel to those in the rest of the nation in the absence of the intervention (access to home equity), so that the trend of the remaining states can serve as valid counterfactual trend for Texas' LFPR in the post-treatment period.

Panel A of Table 3 reports results for the conventional DID specification in Eq. (1). Column (1) shows estimates from the basic DID model with just state and time-fixed effects, without other covariates. Relative to the pre-treatment period (1992–1997), the LFPR in Texas declined about 1 percentage point more than in the remaining states ($\hat{\beta}^{HEL} = -1.08$) after the 1997 amendment allowing HEL. The combined impact of HEL and HELOC after 2003 ($\hat{\beta}^{HELOC} = -2.07$) was roughly twice that of HEL. Although conventional standard errors reflect significance, Conley-Taber 90 percent confidence intervals for $\hat{\beta}^{HEL}$ include zero.[13]

To account for region-specific macro shocks, column (2) includes census division-by-year effects and shows that results remain qualitatively similar to the basic specification in column (1). However, results are quite sensitive to the inclusion of state-specific linear time trends in column (3); point estimates are lower, though Conley-Taber confidence intervals for the effect of HELOC access after 2003 continue to reflect statistical significance.

[13] Confidence intervals are constructed using the procedure in Conley and Taber (2011), who showed that in DID applications with just one treated cluster, conventional standard errors are valid only under the assumption of normality of the error term.

Column (4) drops state-specific linear time trends and instead accounts for differential state-specific trends by controlling for interactions between oil price and state fixed effects. Results remain qualitatively similar to those in column (3), so in all subsequent columns we continue to include state-specific linear time trends.

The DID estimates remain mostly stable in column (5) that adds key state-level economic covariates consistent with theory and state-level demographic covariates.[14] Like previous specifications, Conley-Taber confidence intervals suggest that $\hat{\beta}^{HEL}$ remains negative but imprecisely estimated, while $\hat{\beta}^{HELOC}$ is larger than $\hat{\beta}^{HEL}$ and is statistically significant.

Finally, the specification in column (6) addresses the concern that Texas followed a different timeline from most other states in easing bank branching restrictions following the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994. Unlike most other states, Texas continued to restrict interstate bank branching until 1999. To control for differences in cross-state branching restrictions, we use a time varying index of state-level bank branching restrictions constructed by Rice and Strahan (2010) and recently used in Favara and Imbs (2015). Column (6) of Table 3 suggests that accounting for differences in bank branching restrictions raises the size of the estimated treatment effects both for HEL and HELOC.

To further ease concerns regarding oil price shocks affecting Texas differently from most other states, in Panel B we restrict the sample to the 12 energy-intensive states with more than 1 percent of total employment in mining in the pre-treatment period (1992–1997). The DID estimates are qualitatively similar to those in Panel A and are notably more robust; Conley-Taber confidence intervals suggest that $\hat{\beta}^{HEL}$ and $\hat{\beta}^{HELOC}$ both differ significantly from zero.

[14] Economic covariates are lagged log average hourly wage of manufacturing workers, lagged state income tax rates, lagged log house price and demographic covariates include average age, share female, share white, share black, share married, share of households with children, share with a high school diploma, and share with a college degree.

Heterogeneous DID Estimates

We explore heterogeneity in conventional DID estimates using annual averages of basic monthly CPS data by demographic groups and report the results in Table 4. We present results for the benchmark DID model with census division-by-year effects and other economic and demographic covariates. The main takeaway from Table 4 is that the point estimates are larger for females than males, for the prime-age group relative to the 55+, for the college-educated compared with those lacking college education, and for non-whites relative to whites. It is worth noting that the width of Conley-Taber confidence intervals precludes any definitive conclusions regarding effect heterogeneity across demographic groups.

Nonetheless, differences in point estimates across demographics are broadly in line with intuition. Females may have responded more strongly to relaxed collateral constraints simply because female labor supply is known to be more elastic than male's, particularly on the participation margin. The prime-age group is more responsive to the Texas law change because credit constraints are likely to be more binding for them than for the old. And larger effect for the college-educated, while surprising given that they are less credit-constrained, could stem from their higher homeownership rate and borrowing ability.

Robustness of DID Results

Conventional DID estimates reported in Tables 3 and 4 suggest that access to home equity led to a sharp decline in the LFPR and that the effect with HELOC after 2003 was substantially larger than that with just HEL from 1998–2003. However, DID estimates appear sensitive to state-specific time trends in Panel A of Table 3. [15]

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[15] It is worth noting that a model with state-specific linear time trends may be ill-suited for applications where the law change did not lead to an immediate discrete change in LFPR, but rather to a gradually evolving effect not only on the level of LFPR but also on its growth (Meer and West, 2015; Wolfers, 2006; Lee and Solon, 2011). If so, then DID estimates from specifications without state-specific time trends may actually be more meaningful.

A potential explanation could be that the pre-treatment trends for Texas differ from the rest of the nation and the parallel trends assumption is violated because, by equally weighting diverse states, the DID approach is unable to generate a valid counterfactual trend for the treated state. To informally address this concern, we examine the robustness of our DID estimates to two different approaches.

First, we restrict the estimation sample to just the counties bordering Texas, assuming that trends in counterfactual outcomes for Texas' counties would have been similar to those in contiguous non-Texas counties. Results presented in Appendix Table A1 show that DID estimates are qualitatively similar to those in Table 3. The richest specification in column (3) includes a full set of county-pair by year interactions, so that the DID estimates are identified by within contiguous county-pair variation in home equity access (Dube, Lester, and Reich, 2010).

Secondly, using panel data from the PSID, we estimate the DID specification with individual fixed effects and provide separate estimates for homeowners and renters. The results reported in Appendix Table A2 suggest that, while the full sample results in column (1) mostly echo previous DID results, almost all of the labor supply response was concentrated among homeowners (Table A2 column 2) rather than renters (Table A2 column 3).[16]

While it is reassuring to note that these robustness exercises yield results qualitatively similar to those using state-level DID estimates, we show later in the paper that, by equally weighting all units, DID is unable to achieve parallel trends between Texas and units in the control group. Therefore, in the remainder of the paper we focus on estimation based on synthetic control

[16] The difference in estimated effects between homeowners and renters corresponds to a triple-difference estimate of the effect on homeowners using renters as a control group. In Panel B, column (2) and (3) provide within-Texas before-after estimates and the difference between them yields an alternative DID estimate using the change in LFPR of renters as the counterfactual change in LFPR of homeowners absent intervention.

methods first developed in Abadie et al. (2010)—considered the gold standard for applications with just one treated group.

4.2 Standard Synthetic Control Specifications

Unlike DID, which requires time-constant state effects (α_s), the standard synthetic control method (henceforth SCM-ADH) estimator allows time-varying state effects. The no-treatment counterfactual follows an unobserved common factor model:

$$Y_{st}^{N} = \boldsymbol{X}_{st}\gamma_t + \delta_t + \mu_t\alpha_s + \eta_{st}, \tag{2}$$

where μ_t are common factors and α_s their loadings. Let $t = 1 \dots T_0$ denote the pre-treatment period and $t = T_0 + 1 \dots T$ the post-treatment period. Using some weighted average of control states to estimate $\hat{Y}_{TXt}^{N}$ (henceforth "synthetic Texas"), the treatment effect for Texas ($s = TX$) is recovered as the difference between the actual outcome for Texas *minus* "synthetic Texas".

$$\hat{\beta}_{TX}^{t} = Y_{TXt} - \hat{Y}_{TXt}^{N} = Y_{TXt} - \sum_{s \neq TX} w_s Y_{st} \tag{3}$$

Subject to standard SCM-ADH assumptions, Texas minus "synthetic Texas" gap for $t > T_0$, $\hat{\beta}_{\mathrm{TX}}^{\mathrm{t,Post}}$, yields an unbiased estimate of the treatment effect. With the vector of pre-treatment characteristics of the treated state, $\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}}$, and the matrix for control states, $\mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}$, the vector of weights $\mathbf{W}$ are chosen to minimize $\left\|\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W}\right\|$, subject to the constraint that the weights are non-negative and sum to 1.[17]

[17] $\left\|\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W}\right\| = \sqrt{\left(\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W}\right)'\mathbf{V}\left(\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W}\right)}$, where $\mathbf{V}$ is chosen to minimize the Mean-Squared Prediction error (MSPE) of the outcome variable for the treated state (Texas) in the pre-treatment period, i.e., the mean of the squared deviation between the observed outcome of the treated state (Texas) and its synthetic control. All analysis using synthetic control estimation is carried out using "Synth" package and "Synth Runner" packages (Abadie at al. 2014; Galiani and Quistorff, 2017).

Although $\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}}$ may include linear combinations of the outcome variable (LFPR) and other covariates correlated with the LFPR, the most obvious choice is to use the entire path of pre-treatment lags of the outcome variable ($\mathbf{Y}^{\mathbf{Pre}}$) and minimize $\left\|\mathbf{Y}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Y}_{-\mathbf{TX}}^{\mathbf{Pre}}\mathbf{W}\right\|$, in which case other covariates are redundant. Following Doudchenko and Imbens (2016), in the remainder of the paper, we refer to the model with all pre-treatment lags as the constrained regression model.

Estimates from this model are presented in Figures 2A and 2B. Figure 2A shows that the pre-treatment path of the LFPR for "synthetic Texas" is almost identical to that for Texas, yet the post-treatment paths diverge significantly. Reporting estimated treatment effects, $\hat{\beta}_{\mathrm{TX}}^{\mathrm{t,Post}}$, column (1) of Table 5 shows that the LFPR declined about 0.3 percentage points in 1998, i.e., the first year of access to home equity. The gap widened to -0.8 percentage points 4 years after treatment and then subsided to -0.5 percentage points by the sixth year, in 2003. The Texas *minus* "synthetic Texas" gap widened further after HELOC became available in 2004 and reached 2.6 percentage points 10 years after the 1997 amendment. Estimated weights ($\widehat{\mathbf{W}}$) for control states are reported in Appendix Figure A2.

Since Texas was the only treated state with the law change, control states serve as placebos and should not exhibit post-treatment gaps with respect to their synthetic counterparts that look like Texas. This forms the basis for informal placebo inference presented in Figure 2B. Plots of $\hat{\beta}_{\mathrm{PL}}^{\mathrm{t,Post}}$ for placebo states along with $\hat{\beta}_{\mathrm{TX}}^{\mathrm{t,Post}}$ plotted in solid bold show that just a handful of placebo states have differences as negative as Texas.

Match qualities of pre-treatment LFPR trends among states with respect to their synthetic counterparts, $\hat{\beta}_{\mathrm{PL}}^{\mathrm{t,Pre}}$, differ widely across states. Comparing post-treatment trends for Texas with those of placebo states may not yield the most valid inference if pre-treatment match quality differs between Texas and control states (Abadie et al., 2015; Cavallo Galiani, Noy, & Pantano, 2013).

Using pre-treatment Root Mean Squared Prediction Error ($\text{RMSPE}^{\text{Pre}}$)—calculated as $\sqrt{1/T_0 \sum_{t \le T_0} \left(\hat{\beta}^{\,\text{t,Pre}}\right)^2}$—as a measure of match quality, one solution is to conduct inference based on standardized 2-sided p-values:

$$\text{P-value}_t^{\text{std}} = \Pr\left(\frac{\left|\hat{\beta}_{\text{PL}}^{\text{t,Post}}\right|}{\text{RMSPE}_{\text{PL}}^{\text{Pre}}} \ge \frac{\left|\hat{\beta}_{\text{TX}}^{\text{t,Post}}\right|}{\text{RMSPE}_{\text{TX}}^{\text{Pre}}}\right) \tag{4}$$

Standardized p-values reported in square brackets in column (1) of Table 5 suggest that standardized $\left|\hat{\beta}_{\text{TX}}^{\text{Post}}\right|$ for Texas is the most extreme of all states, yielding p-values of zero. The standardized p-value for the post-treatment average effect for Texas, $\overline{\hat{\beta}_{\text{TX}}^{\text{Post}}}$, reported in the bottom panel of Table 5, also is an extreme outlier among all states.[18] In contrast, the p-value calculated similarly for the pre-treatment average effect, $\overline{\hat{\beta}_{\text{TX}}^{\text{Pre}}}$, equals 1, suggesting that the pre-treatment difference in outcomes between Texas and its counterfactual is not significantly different from those for other states.

To address concerns that SCM-ADH specifications based on all pre-treatment lags may be subject to overfitting, column (2) of Table 5 reports analogous SCM-ADH estimates from a specification that generates synthetic counterfactuals based on using just three pre-treatment lags of LFPR and other covariates guided by theory—the log of state-level average of wage rate, average tax rate, and the log of house price.[19] Estimated treatment effects are larger than those

[18] Standardized p-value for $\overline{\hat{\beta}_{\text{TX}}^{\text{Post}}}$ are based on $\frac{\text{RMSPE}_{\text{TX}}^{\text{Post}}}{\text{RMSPE}_{\text{TX}}^{\text{Pre}}}$, where $\text{RMSPE}^{\text{Post}} = \sqrt{\frac{1}{T-T_0} \sum_{T_0+1 \le t \le T} \left(\hat{\beta}^{\text{t,Post}}\right)^2}$. Appendix Figure A3 plots the normalized average post-RMSE for Texas along with that of other states and shows that Texas is an extreme outlier.

[19] As noted before, Zevelev (2021) found that house prices rose in Texas in response to the 1997 law change. To shed further light on the role of house prices as a potential mechanism for the impact of the Texas law change, we applied the synthetic control method on an alternative measure of the outcome variable, obtained by partialling out the effect of house prices. Comparing synthetic control estimates from this alternative LFPR measure with the traditional estimates can provide informal evidence on the role of house prices. Because leisure is a normal good, part of the labor supply decline after the law change could have been through rising house prices, so controlling for house prices should lower our estimates. As shown in Appendix Figure A4, synthetic control estimates based on LFPR with house price

from the constrained regression model in column 1 and standardized p-values somewhat higher. The 10-year average post-treatment effect reported in the bottom panel is -1.6 percentage point, higher than -1 percentage point in column (1) for the constrained regression model, though the overall pattern of estimated treatment effects is qualitatively similar.[20]

Column (3) of Table 5 reports SCM-ADH estimates with the donor pool limited to energy states, to better control for differential trends due to oil price shocks. Once again, the overall pattern of dynamic effects over time is similar to columns (1) and (2). The average post-treatment effect in the bottom panel is -1.3 percentage points, which is significant at 10 percent level, with a p-value of 0.09.

Robustness of SCM-ADH estimates

To examine robustness to alternative donor pools, we limited the donor pool to states that were similar to Texas in terms of major factors affecting the labor market in the post-treatment period: (1) states that did not change their minimum wage like Texas; (2) states without state-EITC; and (3) states with similar welfare reform policies.[21] Figure 3 shows that the estimated treatment effects are qualitatively similar across alternative donor pools.

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partialled out yield somewhat smaller labor supply reduction, confirming that at least part of the effect of easier home equity access on LFPR operated through higher house prices.

[20] Although not included in the paper due to space constraints, placebo estimates for the synthetic control model in column (2) of Table 5 also showed that $\hat{\beta}_{TX}^{t,Post}$ are unusually negative.

[21] States that kept their minimum wage equal to the federal minimum wage between 1992 and 2007—AL, GA, ID, IN, KS, KY, LA, MS, ND, NE, NM, OK, SC, SD, TN, TX, UT, VA, WY—are from "State Minimum Wage Rates: 1983-2014", retrieved from https://www.taxpolicycenter.org/statistics/state-minimum-wage-rates-1983-2014. States without state-EITC—AK, AL, AR, AZ, CA, CT, FL, GA, HI, ID, KY, LA, MI, MN, MO, MS, MT, NC, ND, NH, NM, NV, OH, PA, SC, SD, TN, TX, UT, WA, WV, WY—are sourced from "State EITC provisions 1977-2016", retrieved from users.nber.org/~taxsim/state-eitc.html. States similar to Texas in terms of the change in cumulative cash welfare during the first 24 months of work between 1996 and 2000 are from Table 2 of Blank (2002) and consist of AK, AL, DE, FL, IA, IL, KS, KY, LA, MI, MO, MS, MT, NC, ND, NJ, NV, OH, PA, SC, TN, TX, UT, VT. To address the concern that a more modest housing boom in Texas could have differentially affected labor market opportunities for young adults relative to the rest of the U.S. (Charles, Hurst, and Notowidigdo, 2017), we also restricted the donor pool to states with house price growth between 2000 and 2006 in the same (bottom) quartile as Texas, and found that the estimated decline in LFPR was even larger.

Additionally, to get a sense of the treatment effect for HELOC, separately from HEL, Figure 4A and 4B plot SCM-ADH estimates analogous to Figures 2A and 2B, using 1998–2003 as the pre-treatment and 2004–2007 as the post-treatment period. They show that the Texas vs. synthetic Texas LFPR trends diverged even more markedly after HELOC became available in 2004, and $\hat{\beta}_{\mathrm{TX}}^{\mathrm{t,Post}}$ lies further into the bottom tail among placebo estimates.[22]

4.3 Synthetic Control Methods Based on Machine Learning

Although the traditional SCM-ADH remains overwhelmingly popular in settings with just one treated cluster, recent work has shown that relaxing some of its implicit restrictions can reduce bias and incorporating insights from machine learning can alleviate concerns of overfitting. In a recent paper, Doudchenko and Imbens (2016) showed that both the DID and SCM-ADH estimators are nested within a more general framework to estimate the treatment effect, $\beta_{TX}^{t} = Y_{TXt} - Y_{TXt}^{N}$ by estimating the missing counterfactual (Y_{TXt}^{N}) using some weighted linear combination of pre-treatment outcomes for all the control states:

$$\hat{Y}_{TXT}^{N} = \kappa + \sum w_i Y_{iT} \quad (5)$$

The intercept (κ) and the weights (w_i) can be thought of as estimates from an OLS regression of pre-treatment outcomes for the treated group (Texas) on the pre-treatment outcomes of 49 remaining control states. If the number of pre-treatment periods is small relative to the number of control states, as is typically the case, then such a regression must impose some restrictions for the intercept and the weights to be even feasible. Identifying four such restrictions: (1) zero intercept ($\kappa = 0$), (2) adding up ($\sum w_i = 1$), (3) non-negative weights ($w_i > 0$), and (4) constant weights

[22] Analogous to Appendix Figures A2 and A3, Appendix Figures A5 and A6 plot weights and normalized post-RMSE, respectively, for the specification with 1998-2003 as the pre-treatment and 2004-2007 as the post-treatment period.

($w_i = \bar{w}$), Doudchenko and Imbens (2016) showed that the DID imposes the last three restrictions and the SCM-ADH imposes the first three. They argue that some of the restrictions may be implausible and relaxing them may reduce bias.[23]

Synthetic Control Method with Elastic Net Penalty

Doudchenko and Imbens (2016) proposed a comprehensive data-driven procedure to relax these restrictions and estimate the intercept and weights using a regularized least-squares model with elastic net shrinkage penalty to minimize the distance between the pre-treatment outcomes of the treated unit and a linear combination of the control units. In the remainder of the paper, we refer to this method as SCM-Elastic Net.

Matrix Completion Method

In another recent paper, Athey et al. (2021) use insights from machine learning and treat the problem of estimating the missing counterfactual for the treated group in the post-treatment period as a matrix completion problem, where the objective is to optimally predict the missing elements of the matrix of outcomes in the non-treated state ($\boldsymbol{Y}$) by minimizing a convex function of the difference between the observed matrix and the unknown complete matrix using nuclear norm regularization. Letting Ω denote the set of row and column indexes, (i, j), of the observed entries of $\boldsymbol{Y}$, and the unknown complete matrix $\boldsymbol{Z}$ to be estimated, the Matrix Completion with Nuclear Norm Minimization (henceforth MC-NNM) objective function can be written as:

[23] For example, Doudchenko and Imbens (2016) noted that the no intercept restriction implies absence of any permanent differences between the treated group and the synthetic control; the adding up constraint is implausible if the treated group is an outlier relative to the control units; and the non-negativity condition helps limit the units with positive weights but may affect out-of-sample predictive ability of the estimated weights and increase bias. Moreover, imposing the first three restrictions may result in non-unique solutions for the intercept and weights if the number of pre-treatment periods is significantly smaller than the number of units, requiring alternative procedures to select among the set of estimated weights.

$$\widehat{\boldsymbol{Z}} = \arg\min_{\boldsymbol{Z}} \sum_{(i,t)\in\Omega} \frac{(Y_{it} - Z_{it})^2}{|\Omega|} + \lambda\|Z\|_*, \tag{6}$$

where $\|Z\|_*$ is the nuclear norm (sum of singular values of $\boldsymbol{Z}$).[24] The regularization parameter, λ, is chosen using five-fold cross-validation. Athey et al. (2021) show that solving for the missing counterfactual using this matrix completion method exploits richer patterns in the data and using extensive simulations they show that the MC-NNM method outperforms both SCM-ADH and SCM-Elastic Net estimators in terms of RMSPE.

Results from SCM-Elastic Net and MC-NNM

Table 6 summarizes the main results from SCM-Elastic Net and MC-NNM in columns (3) and (4), respectively. DID and SCM-ADH models are reported for reference in columns (1) and (2). Estimates from the four models plotted in Figure 5 show that their overall temporal pattern is qualitatively similar to that from the traditional SCM-ADH approach seen earlier, though there are subtle differences across models. Particularly striking is that the equal weighting of control states in the DID model is unable to generate parallel trends between Texas and the control states. This failure of the common trend assumption suggests that DID estimates of the treatment effect are likely biased.

On the other hand, SCM-ADH, SCM-Elastic Net and MC-NNM approaches do a fairly good job of eliminating pre-existing differences between Texas and "synthetic Texas". Analogous to Figure 2B for the traditional synthetic control method, Appendix Figures A7 and A8 plot the estimated effects for Texas alongside effects for placebo states, using SCM-Elastic Net and MC-

[24] Using the algorithm in Mazumder, Hastie, & Tibshirani (2010) MC-NNM starts with the observed matrix with zeros in place of missing entries and iteratively updates the missing entries until convergence, using its singular value decomposition (SVD) with the singular values shrunk by some regularization parameter (λ). Estimation was conducted using software code from https://github.com/susanathey/MCPanel.

NNM, respectively. Estimated treatment effects for Texas are plotted alongside effects for the remaining states used as placebos. Like Figure 2B, they confirm that the post-treatment LFPR decline in Texas was more extreme than in placebo states.[25]

Pre-treatment RMSPEs reported in the bottom panel of Table 6 suggest that MC-NNM by far has the lowest RMSPE for Texas as well as the remainder of placebo states. SCM-ADH matches MC-NNM in pre-treatment fit for Texas but does not do as well for the placebo states. The average treatment effect of a 1.2 percentage point decline in LFPR from the preferred MC-NNM model is somewhat smaller than that from SCM-Elastic Net but larger than the 1 percentage point effect from SCM-ADH. Standardized p-values from MC-NNM are slightly larger than those from the baseline SCM-ADH models reported in column (2), but all estimates are statistically significant. The MC-NNM's p-value of 0.02 for the average effect over 10 years post-treatment indicates that the impact of credit access was significant at conventional levels of significance.

5. Conclusion

We use the 1997 and 2003 constitutional amendments allowing access to home equity borrowing in Texas as natural experiments to estimate the effect of easier credit access on the labor market. Using aggregate state-level as well as micro data, we find that easier access to housing credit led to a notable decline in the LFPR between 1997 and 2007. Employing difference-in-differences and synthetic control methods, we find that the LFPR persistently declined following the amendments allowing home equity loans. Our preferred estimates suggest that easier access to home equity led to a 1.2 percentage point decline in the LFPR, on average, over 10 years.

[25] Appendix Figure A9 plots SCM-ADH estimates along with DID, SCM-Elastic Net, and MC-NNM when restricting the donor pool to energy states and shows that the overall pattern and magnitude of estimated effects are very similar to Figure 5 for the all-states sample.

We find that the LFPR declined by 0.8 percentage points on average between 1997 and 2003, when just HELs and cashout refinancing were available, but the average treatment effect strengthened to 1.8 percentage points after HELOCs were also allowed in 2003. A back-of the-envelope calculation shows that our estimates imply an overall labor supply/earnings response of -2.4 percent between 1997 and 2003, which is about 53 percent of the mid-point estimate of state-level consumption response of the 1997 Texas amendment from Abdallah and Lastrapes (2012). Such a ratio between earnings and consumption responses appears reasonable compared with the 83 percent ratio estimated in Golosov et al. (2021) in response to exogenous unearned income changes.[26]

We show that our estimates are remarkably robust across different synthetic control methods as well as across alternative donor pools. Nonetheless, we may not have captured all remaining differences in LFPR trends between Texas and other states. To that extent, our estimates must be used with caution. For example, complicated changes in means-tested program rules through welfare-to-work reforms and major expansions of the EITC occurred between 1992 and 2007. If Texas responded differently to those changes than other states, and if the timing of those responses were concomitant with the onset of easier home equity access, our estimates may be biased. There may also be some remaining bias due to differential impact of changes in oil prices on Texas vs. the rest of the nation, although our estimates are quite robust to restricting the analysis to the subsample of energy-intensive states.

[26] The -0.8 percentage points extensive margin labor supply response from 1998-2003 equals 1.2 percent of pre-treatment LFPR. Assuming that the extensive margin response accounts for about 50 percent of overall labor supply response and workers affected by the policy had average earnings, the extensive margin response translates into an overall labor supply/earnings response of 2.4 percent, which is about 53 percent of the 4.5 percent mid-point estimate of state-level consumption response in Abdallah and Lastrapes (2012). In making this calculation, we assume that average consumer expenditure and earnings are roughly comparable in dollar terms. For example, according to the BLS Consumer Expenditure Survey, in 2019, the average consumer expenditure per consumer unit was $63,036 and average wages and salaries were $64,708.

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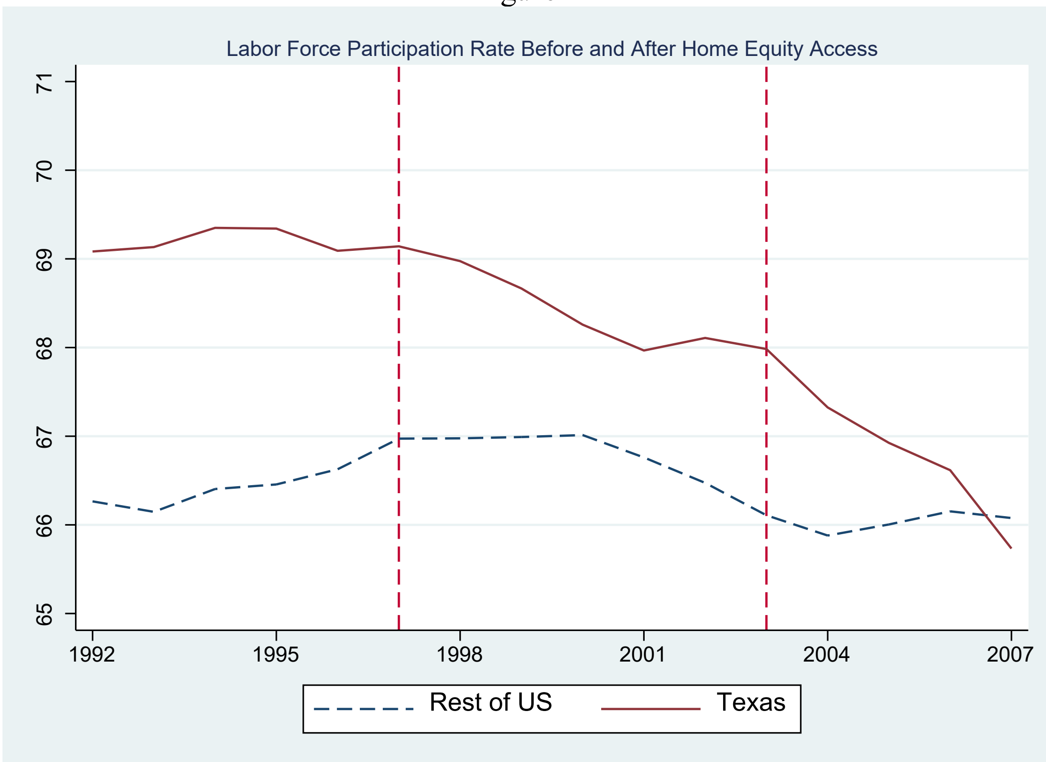
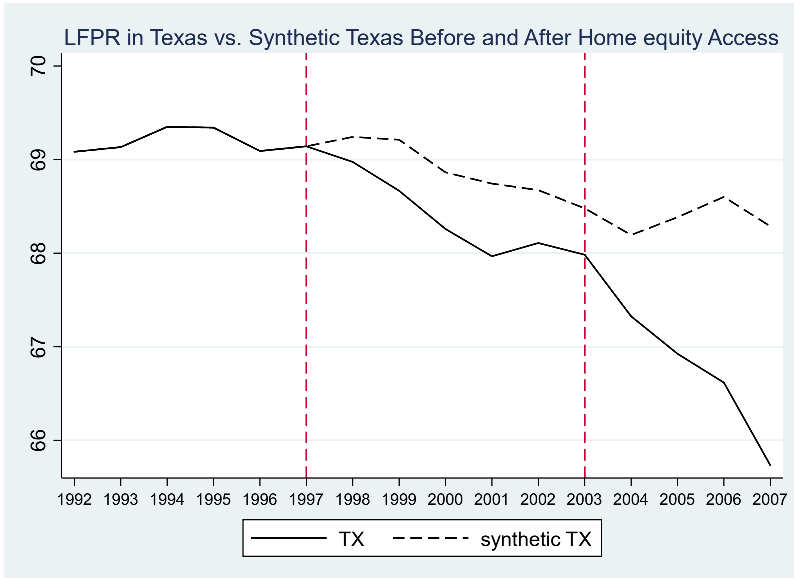
Figure 1



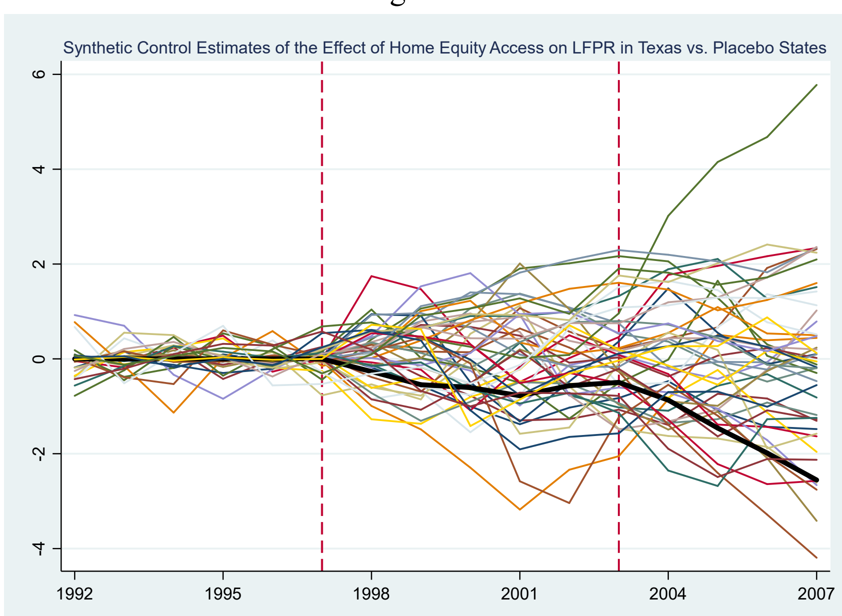
Notes: Using data from BLS-LAUS program, the figure plots state-level LFPR for Texas and the weighted-average LFPR (weighted by population) for the remaining states. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. Sources: BLS/LAUS; Authors' calculations.

Figure 2A



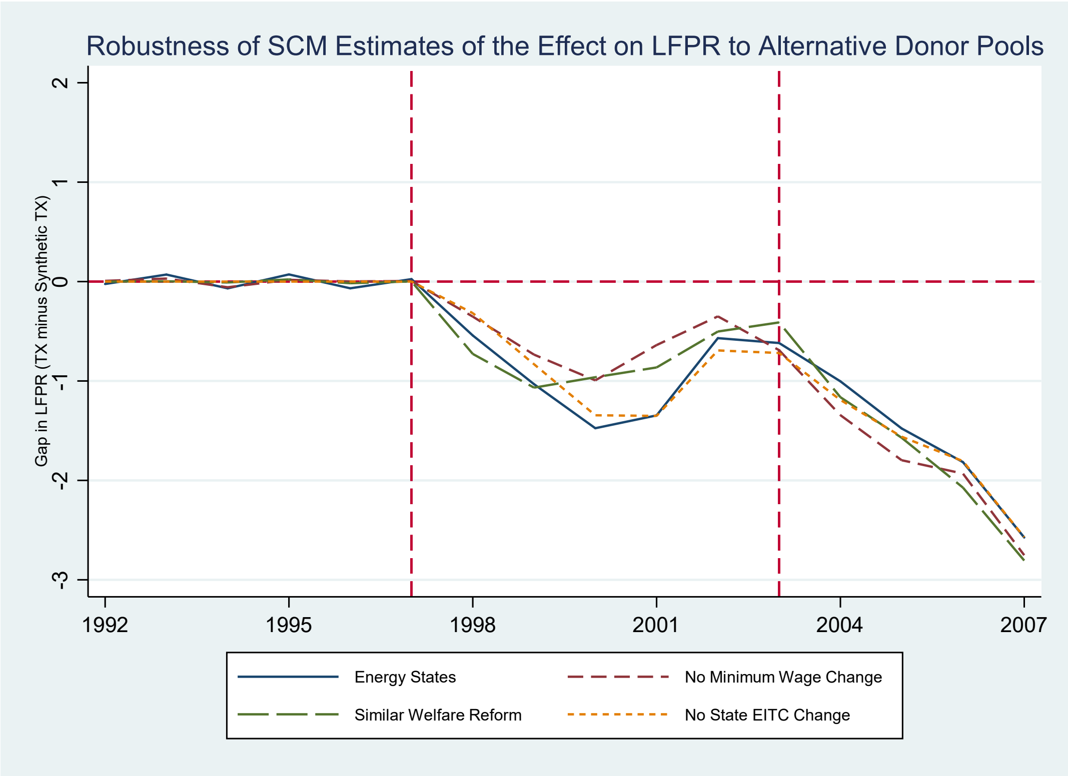
Notes: The figure shows the pre-HEL (1992-1997) and post-HEL (1998-2007) LFPR path for Texas and synthetic Texas using the SCM-ADH specification with all pre-treatment lags of LFPR to construct synthetic Texas. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. The figure shows that the pre-treatment path of LFPR of Texas is almost identical to that for synthetic Texas, yet the post-treatment paths diverge significantly. Estimation carried out using "Synth" package and "Synth Runner" packages (Abadie at al. 2014; Galiani and Quistorff, 2017). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 2B



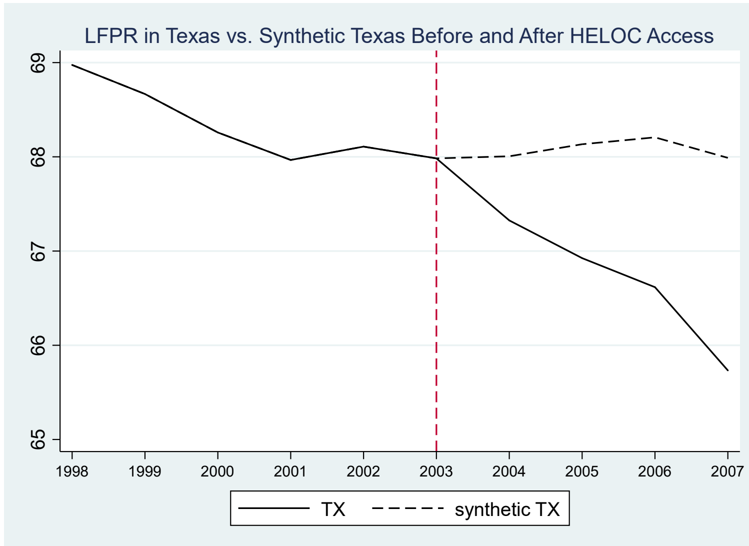
The figure plots the difference between LFPR paths of each state and its synthetic control for the specification described in notes to Figure 2A, with the difference between Texas and synthetic Texas presented in solid bold. The figure shows that just a handful of placebo states have post-treatment LFPR relative to their synthetic counterparts as negative as Texas. Estimation carried out using "Synth" package and "Synth Runner" packages (Abadie at al., 2014; Galiani and Quistorff, 2017). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 3



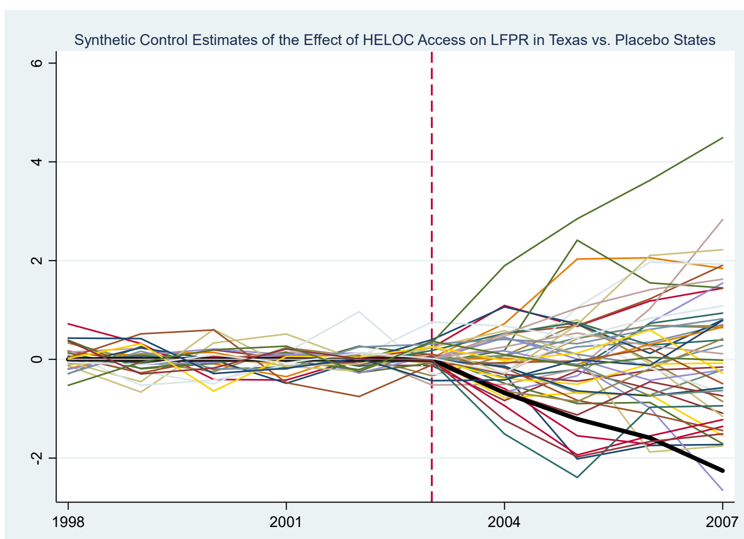
Notes: For alternative donor pools, the figure plots the difference between LFPR paths of Texas and synthetic Texas using the SCM-ADH specification with all pre-treatment lags of LFPR to construct synthetic Texas. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. The figure shows that the pre-treatment path of LFPR of "synthetic Texas" is almost identical to that for Texas, yet the post-treatment paths diverge significantly for all four alternative donor pools. Estimation carried out using "Synth" package and "Synth Runner" packages (Abadie at al. 2014; Galiani and Quistorff, 2017). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 4A



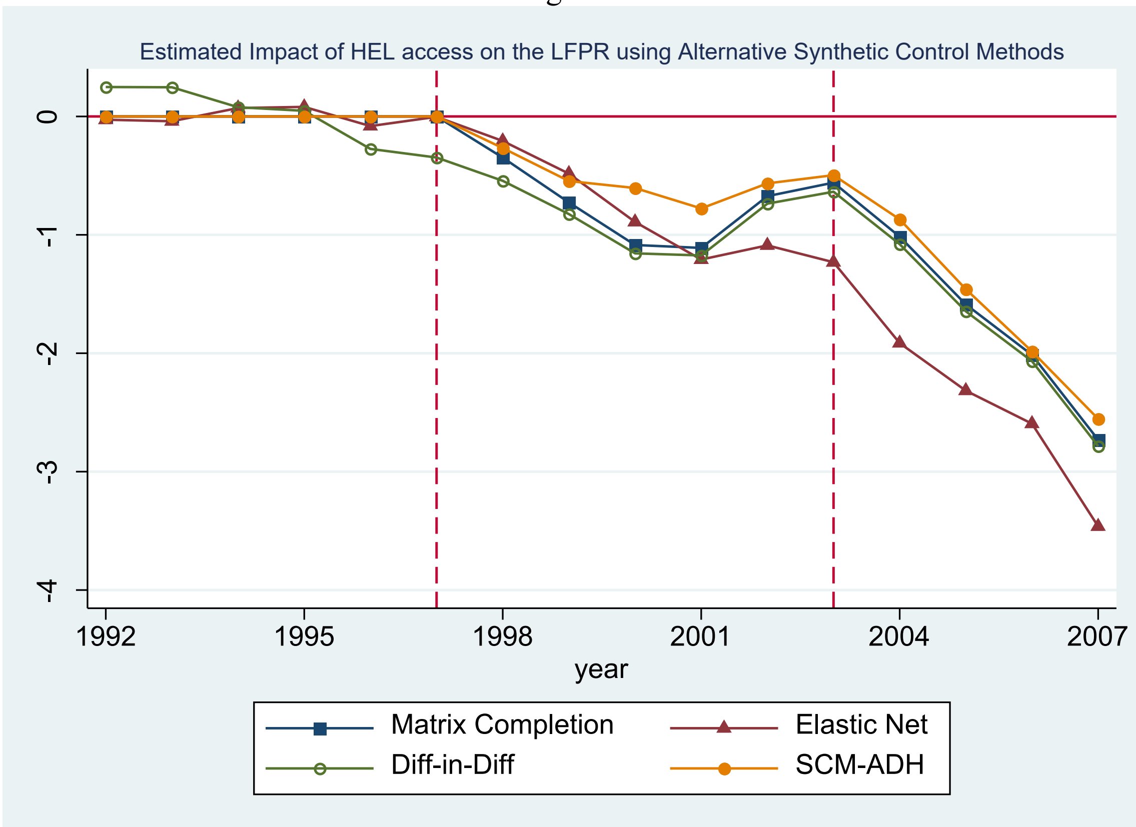
The figure shows the pre-HELOC (1998-2003) and post-HELOC (2004-2007) LFPR path for Texas and synthetic Texas using the SCM-ADH specification with all pre-treatment lags of LFPR to construct synthetic Texas. Vertical dashed line denotes 2003, the year of introduction of HELOC. The figure shows that the pre-HELOC path of LFPR of "synthetic Texas" is almost identical to that for Texas, yet the post-HELOC paths diverge significantly. Estimation carried out using "Synth" package and "Synth Runner" packages (Abadie at al., 2014; Galiani and Quistorff, 2017). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 4B



The figure plots the difference between LFPR paths of each state and its synthetic control for the specification described in notes to Figure 4A, with the difference between Texas and synthetic Texas presented in solid bold. The figure shows that just a handful of placebo states have post-treatment LFPR relative to their synthetic counterparts as negative as Texas. All analysis using synthetic control estimation is carried out using "Synth" package and "Synth Runner" packages (Abadie at al. 2014; Galiani and Quistorff, 2017). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 5



Notes: The figure plots the pre-HEL (1992-1997) and post-HEL (1998-2007) difference between LFPR paths for Texas and synthetic Texas using different synthetic control methods and the specification with all pre-treatment lags of LFPR to construct synthetic Texas. The estimates plotted are reported in Table 6. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. The figure shows that the pre-treatment path of LFPR of Texas is mostly identical to that for synthetic Texas for all synthetic control methods (but not for DID), yet the post-treatment paths diverge significantly. Estimation carried out using software code for DID/SCM-ADH/SCM-Elastic Net/MC-NNM available from https://github.com/susanathey/MCPanel. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Table 1: Impact of Texas Home Equity Amendments on Loan Originations

	(1)	(2)	(3)
	Panel A: Log (Number of Loan Originations)		
	Cash-out	HEL	HELOC
Texas X Post 1997	0.740	0.073	
	(0.088)	(0.025)	
	[0.306, 1.392]	[-0.144, 0.258]	
Texas X Post 2003			0.900
			(0.068)
			[0.650, 1.273]
Observations	300	300	350
Adj R-Sq	0.964	0.992	0.984
	Panel B: Log (Amount of Loan Originations)		
Texas X Post 1997	0.415	0.217	
	(0.056)	(0.028)	
	[0.040, 1,015]	[-0.007, 0.458]	
Texas X Post 2003			0.550
			(0.071)
			[0.305, 0.949]
Observations	300	300	350
Adj R-Sq	0.947	0.961	0.968
Estimation Period	1995-2000	1995-2000	2001-2007
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Demographics	Yes	Yes	Yes

Notes: Robust standard errors clustered by state are reported in parenthesis; Conley-Taber confidence intervals in square brackets. Estimation is weighted by state population. The table reports DID coefficients from a regression of log number of loans originated in Panel A (amount in Panel B) on the interactions between the treatment dummy (an indicator for Texas) and dummies for 1998-2003 and 2004-2007, controlling for state and year fixed effects and other state-level demographic covariates: average age, and state's share of population that is female, married, white, black, with a high school diploma, and with a college degree. Sources: FRBNY Consumer Credit Panel/Equifax Data; BKFS; Basic Monthly CPS; Authors' calculations.

Table 2: Summary Statistics

	Pre-Treatment (1993-1997)		Post-Treatment (1998-2007)	
	Rest of US	Texas	Rest of US	Texas
LFPR	66.48	69.19	66.43	67.61
	(3.452)	(0.122)	(3.129)	(1.007)
Log Real Wage*	2.971	2.896	2.989	2.864
	(0.114)	(0.0144)	(0.108)	(0.0481)
Avg. State Tax rate	0.0218	0	0.0230	0
	(0.00975)	(0)	(0.0107)	(0)
Log FHFA HPI	5.258	4.882	5.698	5.206
	(0.206)	(0.0428)	(0.343)	(0.133)
Age	43.35	41.37	44.30	42.37
	(1.142)	(0.175)	(1.202)	(0.522)
Share Female	0.521	0.514	0.519	0.514
	(0.00909)	(0.00207)	(0.00809)	(0.00178)
Share Married	0.565	0.586	0.549	0.571
	(0.0244)	(0.00398)	(0.0241)	(0.00574)
Households with Children	0.331	0.376	0.316	0.360
	(0.0210)	(0.00644)	(0.0207)	(0.0129)
Share White	0.759	0.581	0.717	0.515
	(0.123)	(0.0145)	(0.134)	(0.0187)
Share Black	0.113	0.111	0.113	0.109
	(0.0787)	(0.00215)	(0.0778)	(0.00427)
Share High School Grad	0.334	0.291	0.315	0.274
	(0.0435)	(0.00736)	(0.0443)	(0.00537)
Share College Grad	0.202	0.186	0.239	0.214
	(0.0356)	(0.00509)	(0.0402)	(0.00703)

Note: Using state-level data the table presents means, with standard deviation in parenthesis. *Log real wage are for workers in manufacturing. Sources: BLS/LAUS; Authors' calculations.

Table 3: Difference in Differences Estimates of Effects of Home Equity Access on LFPR

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: All States Sample			
Texas X 1998-2003	-1.080	-0.717	-0.085	-0.501	-0.501	-0.884
	(0.144)	(0.649)	(0.686)	(0.696)	(0.436)	(0.378)
	[-1.723, 0.140]	[-1.465, 0.349]	[-0.871, 1.001]	[-1.487, 0.557]	[-1.205, 0.096]	[-1.346, -0.433]
Texas X Post 2003	-2.069	-2.625	-1.474	-0.935	-1.270	-1.901
	(0.219)	(0.300)	(0.541)	(0.464)	(0.683)	(0.669)
	[-3.811, -0.808]	[-4.536, -1.269]	[-2.324, -0.945]	[-1.213, -0.730]	[-1.734, -0.728]	[-2.350, -1.530]
Observations	800	800	800	800	797	597
Adj R-Sq	0.943	0.951	0.965	0.964	0.978	0.984
			Panel B: Energy States Sample			
Texas X 1998-2003	-1.152	-1.310	-0.714	-1.115	-0.833	-0.983
	(0.152)	(0.380)	(0.104)	(0.419)	(0.362)	(0.615)
	[-2.012, -0.595]	[-2.509, -0.850]	[-1.283, -0.195]	[-2.064, -0.883]	[-1.360, -0.382]	[-1.516, -0.524]
Texas X Post 2003	-2.357	-2.888	-1.796	-1.360	-1.573	-1.954
	(0.290)	(0.145)	(0.681)	(0.054)	(0.845)	(1.109)
	[-5.826, -1.759]	[-4.830, -2.070]	[-2.208, -1.519]	[-1.589, -1.030]	[-1.929, -1.226]	[-2.231, -1.651]
Observations	192	192	192	192	192	144
Adj R-Sq	0.979	0.982	0.984	0.985	0.986	0.988
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Div. X Year Effects	No	Yes	Yes	Yes	Yes	Yes
State X Linear Trend	No	No	Yes	No	Yes	Yes
Oil Price X State FE	No	No	No	Yes	No	No
Other Controls	No	No	No	No	Yes	Yes
Bank Branching	No	No	No	No	No	Yes

Notes: Robust standard errors clustered by state are reported in parenthesis. 90 percent confidence intervals using Conley and Taber (2011) reported in square brackets. Estimation is weighted by state population. Using state-level data from 1992-2007, the table reports coefficients on the interactions Texas X 1998-2003 and Texas X Post-2003 dummies from a DID regression of the LFPR on the interactions, state fixed effects, year fixed effects (in column 1), and other controls, as indicated, in column 2-4. Other state-level covariates included are—lagged log average hourly wage of manufacturing workers, lagged state income tax rates, lagged log house price and state-level demographic covariates—average age, share female, share white, share black, share married, share of households with children, share with a high school diploma, and share with a college degree. We end up with 797 observations in models with covariates due to missing manufacturing wage data for Delaware from 2003-2005. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Table 4: Heterogeneity in DID Estimates of Effects of Home Equity Access on LFPR

	(1) Male	(2) Female	(3) Prime-Age	(4) Age-55+	(5) No-College	(6) Any-College	(7) White	(8) Non-White
Texas X 1998-2003	-0.376	-0.639	-1.436	1.477	0.269	-1.886	-0.050	-2.520
	(0.536)	(0.802)	(0.381)	(0.890)	(0.631)	(0.745)	(0.718)	(0.655)
	[-1.226, 1.143]	[-1.671, 0.725]	[-2.312, -0.567]	[-0.659, 3.363]	[-0.847, 1.679]	[-2.845, -0.599]	[-1.032, 0.774]	[-5.217, 0.470]
Texas X Post 2003	-1.165	-2.245	-1.819	0.004	-1.297	-2.671	-1.229	-4.052
	(0.230)	(0.716)	(0.635)	(0.303)	(0.796)	(0.922)	(0.395)	(0.556)
	[-3.313, 0.112]	[-4.352, -0.893]	[-3.485, -0.517]	[-2.755, 3.047]	[-3.496, 0.471]	[-4.714, -1.408]	[-3.537, 0.240]	[-7.668, -0.863]
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Div. X Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63372	65417	48508	41464	67219	61570	38274	57528
Adj R-Sq	0.890	0.891	0.675	0.753	0.876	0.839	0.928	0.770

Notes: Robust standard errors clustered by state are reported in parenthesis. 90 percent confidence intervals using Conley and Taber (2011) reported in square brackets. Estimation is weighted by group-cell count. Using grouped basic CPS data by state, year and demographic groups from 1992-2007, the table reports coefficients on the interactions Texas X 1998-2003 and Texas X Post-2003 dummies from a DID regression of the LFPR on the interactions, state fixed effects, year fixed effects, division X year effects, indicators for demographic groups as other controls. Data Sources: Basic Monthly CPS; Authors' calculations.

Table 5: Standard Synthetic Control Estimates of Effects of Home Equity Access on LFPR with Standardized P-Values

	All Pre-Treatment Lags	Covariates and Some Lags	All Pre-Treatment Lags: Energy States
1998	-0.267	-0.527	-0.542
	[0.000]	[0.163]	[0.091]
1999	-0.545	-0.988	-1.032
	[0.000]	[0.061]	[0.091]
2000	-0.605	-1.566	-1.475
	[0.000]	[0.041]	[0.091]
2001	-0.777	-1.804	-1.346
	[0.000]	[0.082]	[0.091]
2002	-0.565	-1.146	-0.569
	[0.000]	[0.122]	[0.091]
2003	-0.496	-1.181	-0.618
	[0.000]	[0.163]	[0.091]
2004	-0.869	-1.444	-1.003
	[0.000]	[0.061]	[0.091]
2005	-1.459	-1.923	-1.477
	[0.000]	[0.041]	[0.091]
2006	-1.985	-2.240	-1.816
	[0.000]	[0.020]	[0.091]
2007	-2.554	-2.969	-2.574
	[0.000]	[0.020]	[0.091]
Treatment Effect	-1.012	-1.579	-1.245
Standardized P-value	0	0.0408	0.0909
Pre-Mean Effect	9.47e-13	-0.0675	0.00121
Pre-Std. P-value	1	0.857	0.909
Pre-RMSPE: TX	1.96e-10	0.151	0.0586
Pre-RMSPE: Donor Pool	0.309	0.498	1.152

Notes: Standardized P-values reported in square brackets. Pre-treatment period: 1992-1997; Post-treatment period: 1998-2007; Treated group: Texas; Donor pool: 49 remaining states. The table shows synthetic control estimates of the treatment effects, i.e., post-1997 differences in LFPR of Texas and synthetic-Texas. All analysis using synthetic control estimation is carried out using the "Synth" and "Synth Runner" packages (Abadie at al. 2014; Galiani and Quistorff, 2017). Sources: BLS-LAUS; Authors' calculations.

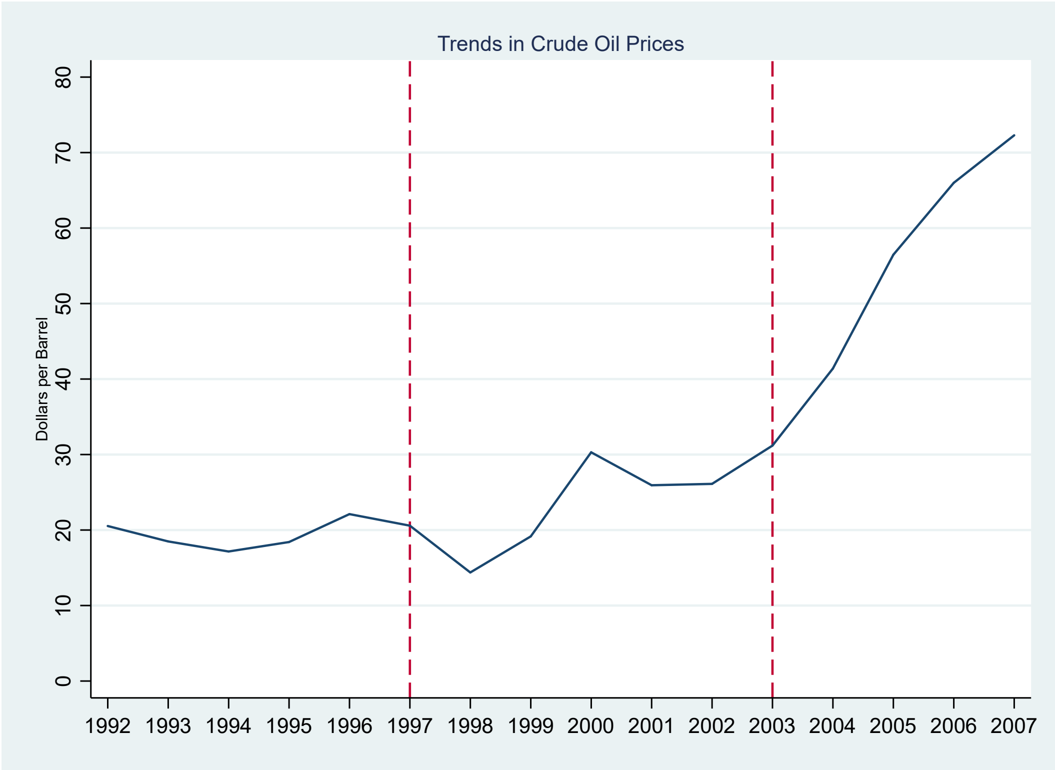
Table 6: Estimated Treatment Effects of Home Equity Access on LFPR from Alternative Synthetic Control Methods with Standardized P-Values

	(1) Diff-in-Diff	(2) SCM-ADH	(3) SCM-Elastic Net	(4) MC-NNM
1998	-0.544	-0.267	-0.207	-0.348
	[0.163]	[0.000]	[0.714]	[0.0204]
1999	-0.824	-0.545	-0.483	-0.726
	[0.0612]	[0.000]	[0.612]	[0.0204]
2000	-1.156	-0.605	-0.891	-1.086
	[0.0612]	[0.000]	[0.510]	[0.0204]
2001	-1.174	-0.777	-1.208	-1.110
	[0.0816]	[0.000]	[0.571]	[0.0204]
2002	-0.736	-0.565	-1.089	-0.673
	[0.245]	[0.000]	[0.510]	[0.0204]
2003	-0.636	-0.496	-1.231	-0.559
	[0.245]	[0.000]	[0.469]	[0.0204]
2004	-1.079	-0.869	-1.914	-1.015
	[0.122]	[0.000]	[0.510]	[0.0204]
2005	-1.646	-1.459	-2.316	-1.588
	[0.0408]	[0.000]	[0.469]	[0.0204]
2006	-2.067	-1.985	-2.595	-2.014
	[0.0204]	[0.000]	[0.469]	[0.0204]
2007	-2.784	-2.554	-3.463	-2.731
	[0.0204]	[0.000]	[0.327]	[0.0204]
Treatment Effect (1998-2004)	-1.265	-1.012	-1.540	-1.185
Standardized P-value	0.0408	0	0.551	0.0204
Treatment Effect (1998-2003)	-0.845	-0.543	-0.852	-0.750
Treatment Effect (2004-2007)	-1.894	-1.717	-2.572	-1.837
Pre-Treatment Mean Effect	0	9.47e-13	1.66e-14	0
Pre-Treatment Std. P-value	0.878	1	0.367	1
Pre-Treatment RMSPE: Texas	0.233	1.96e-10	0.0586	0
Pre-Treatment RMSPE: Donor Pool	0.632	0.309	0.0853	0.0708

Notes: Standardized P-values reported in square brackets. Pre-treatment period: 1992-1997; Post-treatment period: 1998-2007; Treated group: Texas; Donor pool: 49 remaining states. The table shows estimates of the post-treatment difference between LFPR of Texas and synthetic-Texas using all pre-treatment lags of the LFPR to construct the synthetic control for Texas. Estimation carried out using software code for DID/SCM-ADH/MC-NNM code available from https://github.com/susanathey/MCPanel.

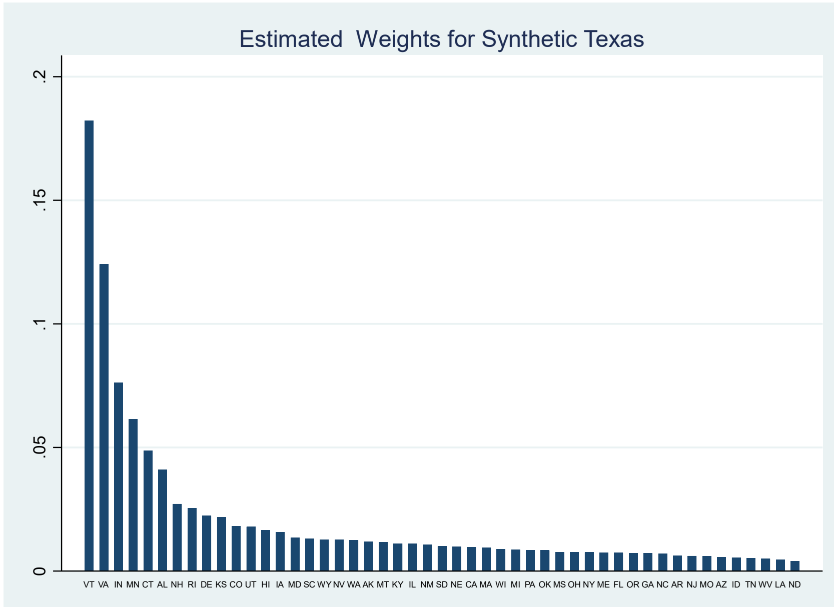
Appendix A

Figure A1



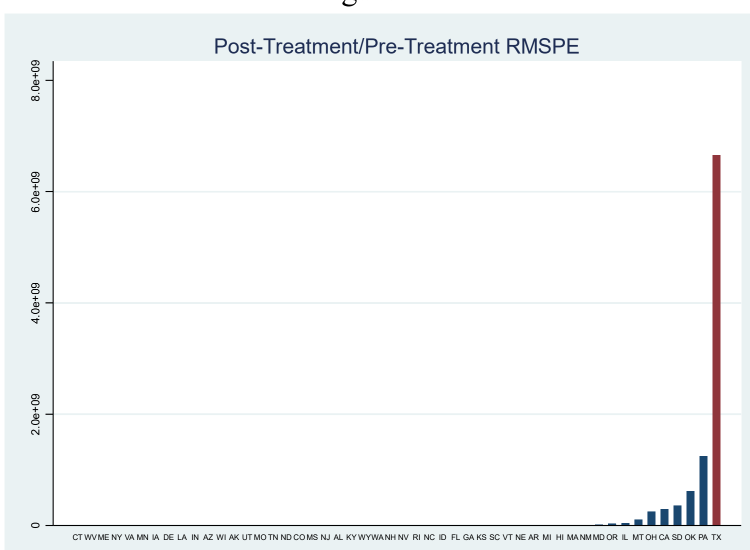
Sources: Department of Energy; Haver Analytics.

Figure A2



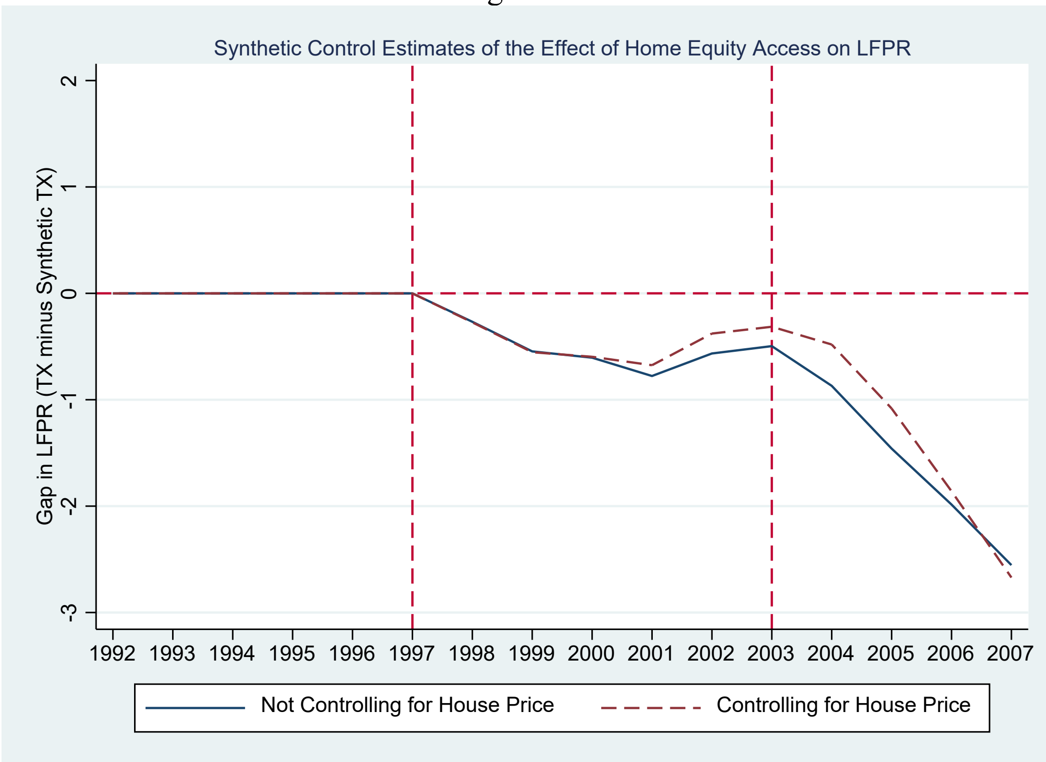
Notes: The figure shows the estimated weights for different states in constructing synthetic Texas for the SCM-ADH estimates plotted in Figure 2A/2B and reported in column (1) of Table 5. See notes to Figure 2A/2B and Table 5 for more details. All analysis using synthetic control estimation is carried out using “Synth” package and “Synth Runner” packages (Abadie at al. 2014; Galiani and Quistorff, 2017). Sources: BLS/LAUS; Authors’ calculations.

Figure A3



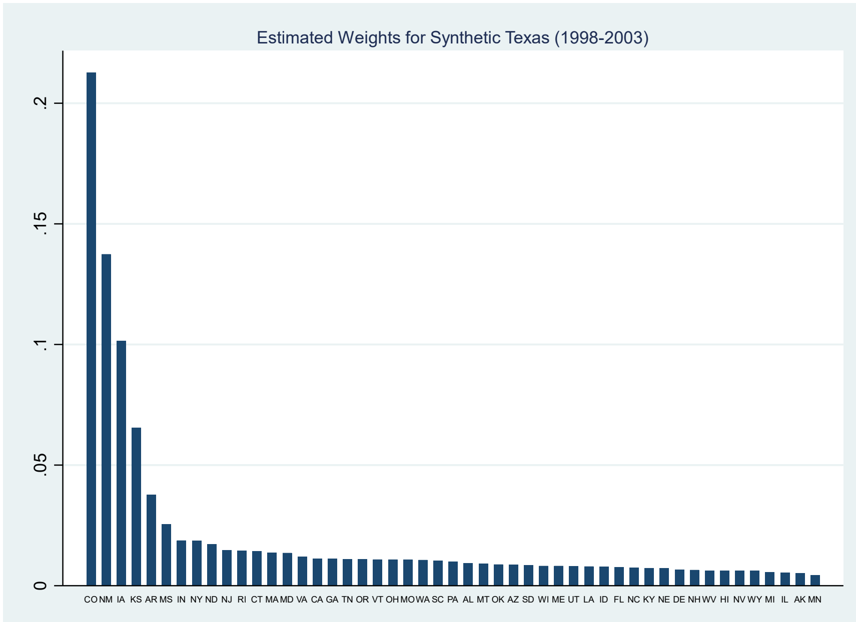
Notes: The figure plots the ratio of post-treatment RMSPE to the pre-treatment RMSPE of Texas and other control states for the SCM-ADH estimates plotted in Figure 2A/2B and reported in column (1) of Table 5. RMSPE for each state is simply the square root of the mean squared difference between the LFPR of that state and the synthetic control for that state. The optimal weights for Texas are shown in Figure A2. The figure shows that the post-treatment difference in LFPR of Texas and synthetic Texas relative to the pre-treatment difference is the largest of all states. Sources: BLS/LAUS; Authors’ calculations.

Figure A4



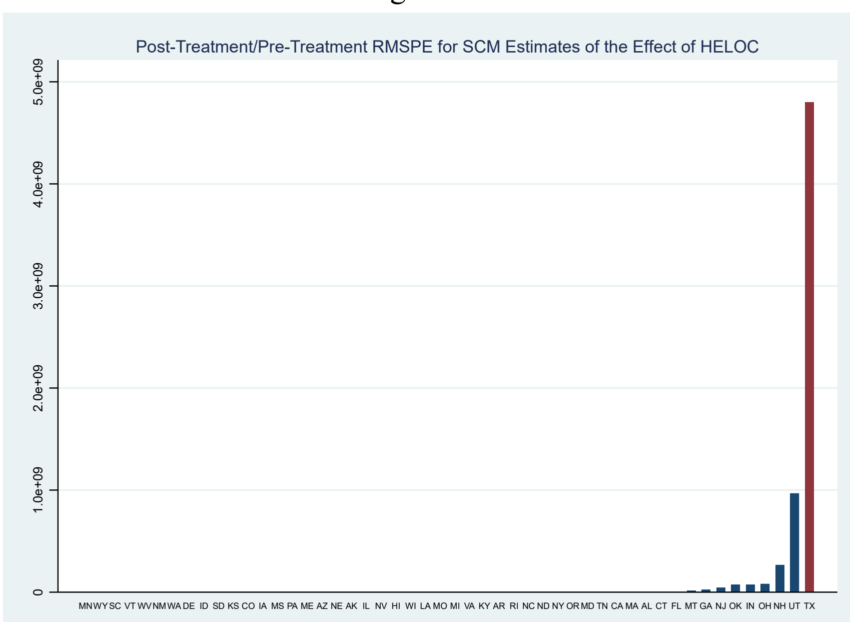
Notes: The figure plots the difference between LFPR paths of Texas and synthetic Texas using the SCM-ADH specification with all pre-treatment lags of LFPR to construct synthetic Texas. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. The figure shows that the pre-treatment path of LFPR of "synthetic Texas" is almost identical to that for Texas, yet the post-treatment paths diverge significantly. The figure shows that synthetic control estimates based on LFPR with house price partialled out (dashed line) yield somewhat smaller labor supply reduction than the baseline specification without adjustment for house prices (solid line). Estimation carried out using "Synth" package and "Synth Runner" packages (Abadie at al. 2014; Galiani and Quistorff, 2017). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure A5



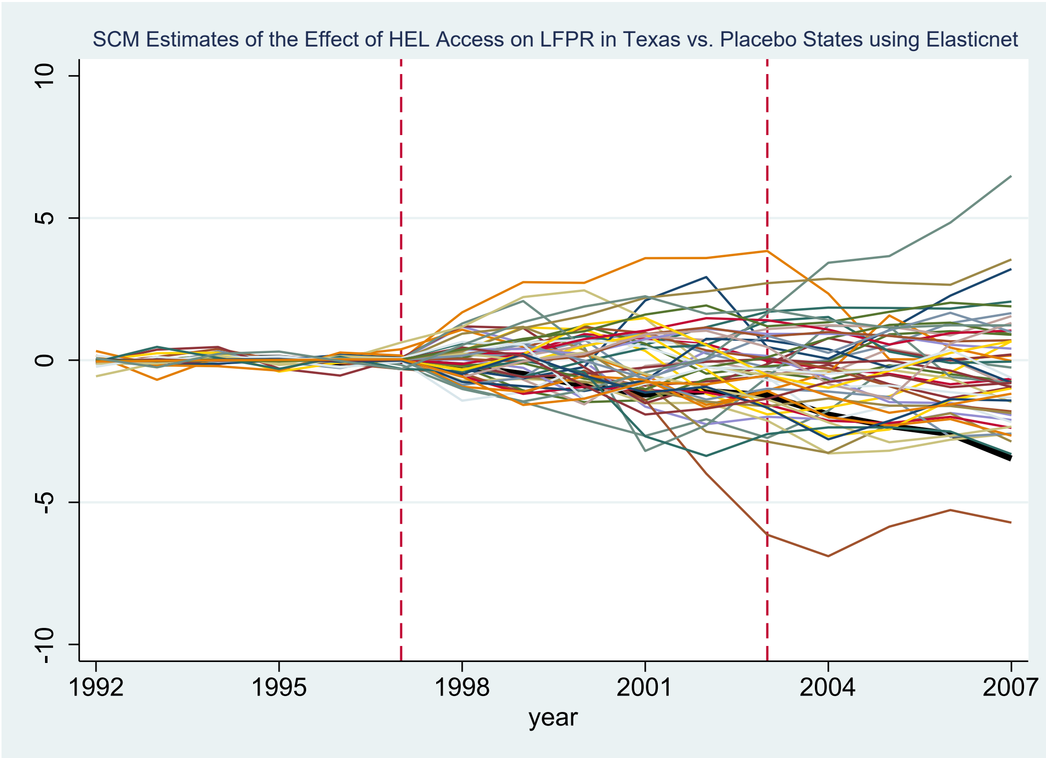
Notes: The figure shows the estimated weights for different states in constructing synthetic Texas for the SCM-ADH estimates plotted in Figure 4A/4B. The figure is analogous to Figure A2, except that it plots estimated weights for SCM-ADH estimated effects of HELOC in the post-2003 period. See notes to Figure A2 for more details.

Figure A6



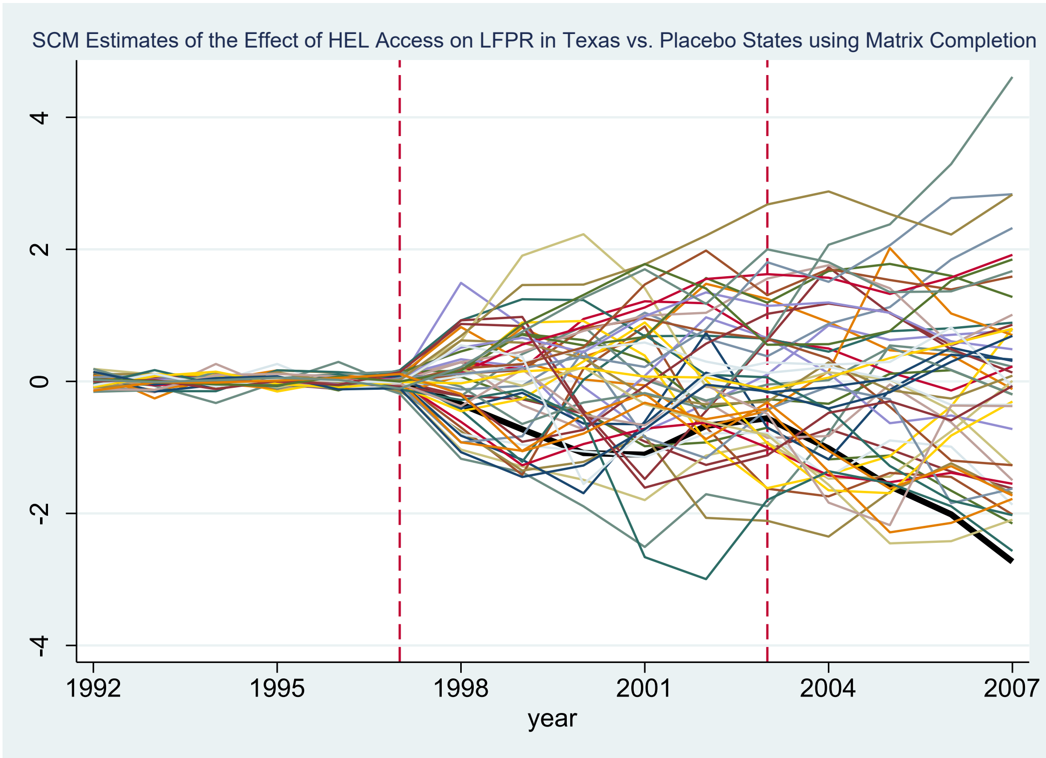
Notes: The figure plots the ratio of post-HELOC (2004-2007) RMSPE to the pre-HELOC (1998-2003) RMSPE of Texas vs. other states for the synthetic control estimates plotted in Figure 4A/4B. The figure is analogous to Figure A3, except that it uses SCM-ADH estimates of HELOC in the post-2003 period. See notes to Figure A3 for more details.

Figure A7



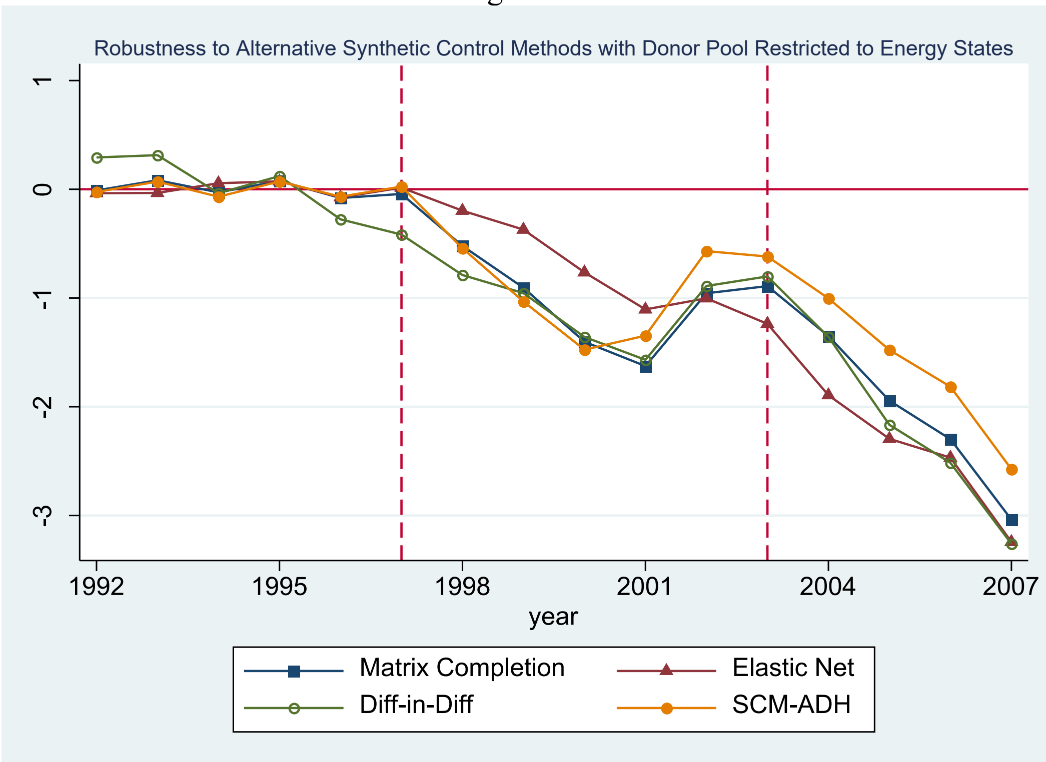
Notes: The figure plots the difference between LFPR paths of each state and its synthetic control for the SCM-Elastic Net model, with the difference between Texas and synthetic Texas presented in solid bold. The figure shows that just a handful of placebo states have post-treatment LFPR relative to their synthetic counterparts as negative as Texas. Estimation carried out using software code for DID/SCM-ADH/SCM-Elastic Net/MC-NNM available from https://github.com/susanathey/MCPanel. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure A8



Notes: The figure plots the difference between LFPR paths of each state and its synthetic control for the Matrix Completion (MC-NNM) model, with the difference between Texas and synthetic Texas presented in solid bold. The figure shows that just a handful of placebo states have post-treatment LFPR relative to their synthetic counterparts as negative as Texas. Estimation carried out using software code for DID/SCM-ADH/SCM-Elastic Net/MC-NNM available from https://github.com/susanathey/MCPanel. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure A9



Notes: The figure plots the pre-HEL (1992-1997) and post-HEL (1998-2007) difference between LFPR paths for Texas and synthetic Texas using different synthetic control methods and the specification with all pre-treatment lags of LFPR to construct synthetic Texas, with the donor pool restricted to energy states. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. The figure shows that the pre-treatment path of LFPR of Texas is mostly identical to that for synthetic Texas for all synthetic control methods (but not for DID), yet the post-treatment paths diverge significantly. Estimation carried out using software code for DID/SCM-ADH/SCM-Elastic Net/MC-NNM available from https://github.com/susanathey/MCPanel. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Table A1: Difference-in-Differences Estimates using only Border Counties

	(1)	(2)	(3)
Texas X 1998-2003	-1.798	-1.117	-2.149
	(1.084)	(0.352)	(1.336)
Texas X Post 2003	-3.699	-2.44	-3.16
	(2.002)	(0.413)	(2.568)
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No
State X Linear Trend	No	Yes	No
County-Pair X Year Effects	No	No	Yes
Observations	2128	2128	2128
Adj R-Sq	0.6091	0.6397	0.6552

Notes: Robust standard errors clustered by county are reported in parenthesis. Estimation is weighted by county population. Using county-level data from 1992-2007, the table reports DID coefficients from a regression of county-level LFPR on the interactions between the treatment dummy (an indicator for Texas) and dummies for 1998-2003 and 2003-2007, controlling various fixed effects as indicated. Estimation sample restricted to contiguous counties around Texas' border with other states. Sources: BLS-LAUS; Authors' calculations.

Table A2: Estimated Effects of Home Equity Access on LFP by Homeowners and Renters using Panel Data Specifications with Individual Fixed Effects

Panel A: Full Sample

	(1) All	(2) Homeowners	(3) Renters
Texas X 1998-2003	-1.561	-2.813	0.660
	(0.471)	(0.539)	(0.704)
Texas X Post 2003	-1.243	-2.316	1.279
	(0.595)	(0.742)	(0.809)
Year Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Bank Branching Control	Yes	Yes	Yes
Observations	159087	104931	54156
Adj R-Sq	0.714	0.718	0.731

Panel B: Texas Sample

	(1) All	(2) Homeowners	(3) Renters
1998-2003	-3.058	-2.363	-1.521
	(1.479)	(1.923)	(2.465)
Post 2003	-3.899	-2.344	-1.492
	(2.286)	(2.976)	(3.793)
Demographic Controls	Yes	Yes	Yes
Bank Branching Control	Yes	Yes	Yes
Observations	9131	5589	3542
Adj R-Sq	0.729	0.734	0.749

Notes: Robust standard errors clustered by state are reported in parenthesis in Panel A and robust standard errors in Panel B. The table presents unweighted estimates from a DID regression of labor force participation dummy (LFP) with individual fixed effects. Other demographic covariates included in columns (1) and (2) are: age, married, dummies for high school diploma, and college degree. Results are based on the entire unbalanced panel from 1992 to 2007 in the PSID. Sources: PSID-CNEF; Authors' calculations.

Appendix B: Theoretical Framework

We extend the standard two-period life-cycle model of Rossi and Trucchi (2016) to a three-period set-up and, following Hurst and Stafford (2004) and Bhutta and Keys (2016), explicitly incorporate home ownership, mortgage borrowing, house price appreciation, home equity extraction, and collateral constraints to capture the key features of the Texas housing market. In our model, the agent chooses consumption (c_t) in the three periods ($t = 1,2,3$), and leisure (l_t), and home equity extraction (E_t) in the first two periods to maximize a three-period intertemporally separable utility function with δ the discount factor:

$$U = u(c_1, l_1) + \delta u(c_2, l_2) + \delta^2 U(c_3, 1)$$

subject to the budget constraints:

$$c_1 = w(1 - l_1) + E_1 - r\pi H_0 - A_1$$

$$c_2 = A_1(1 + r) + w(1 - l_2) + E_2 - (1 + r)E_1 - r\pi H_0 - A_2$$

$$c_3 = P + A_2(1 + r) + [(1 + r_H)^3 H_0 - (1 + r)\pi H_0] - E_2(1 + r)$$

and the collateral constraints:

$$E_1 \leq a(1 + r_H)H_0 - \pi H_0$$

$$E_2 \leq a(1 + r_H)^2 H_0 - \pi H_0$$

To keep the model simple we normalize total time endowment to 1, so that labor supply in the first two periods are $(1 - l_t)$ at wage rate (w), and assume that the agent retires with retirement income P in the third period. Following Hurst and Stafford (2004), at the beginning of the first period, the agent owns a home worth H_0 with an initial LTV (π) financed with an interest-only mortgage that equals πH_0, with a fixed mortgage rate (r). The interest-only mortgage payment each period is πH_0, and the constant rate of house price appreciation is r_H. The agent chooses to extract equity E_t subject to the collateral constraint that total equity extraction *plus* the outstanding mortgage amount cannot exceed some fraction (a) of the current home value. Furthermore, as per Texas law an existing home equity loan must be paid off before another one is taken. The parameter a governs the ease of credit access. It equaled 1 in all other states throughout the sample period from 1992 to 2007—households could borrow the entire home equity—but switched from 0 to 0.8 in Texas after the 1997 amendment. A_1 and A_2 represent savings in the first two periods, respectively. The agent leaves no bequests in period 3 and consumes the proceeds from home sale, $(1+r_h)^3 H_0$, after paying off the interest only mortgage (πH_0) and borrowed equity $E_2(1+r)$. For the three-period model the Lagrangian can be written as is:

$$\max_{\{c_1, l_1, c_2, l_2, c_3, E_1, E_2, \mu_1, \mu_2, \mu_3\}} \quad L = u(c_1, l_1) + \delta u(c_2, l_2) + \delta u(c_3, 1)$$

$$-\mu_1[c_1 - w(1-l_1) - E_1 + r\pi H_0 + A_1]$$

$$-\mu_2[c_2 - (1+r)A_1 - w(1-l_2) - E_2 + (1+r)E_1 + r\pi H_0 + A_2]$$

$$-\mu_3[c_3 - (1+r)A_2 - P - (1+r_H)^3 H_0 + (1+r)\pi H_0 + (1+r)E_2]$$

$$-\mu_4[E_1 - a(1+r_H)H_0 + \pi H_0]$$

$$-\mu_5[E_2 - a(1+r_H)^2 H_0 + \pi H_0]$$

μ_1, μ_2, μ_3, μ_4, and μ_5 are Kuhn-Tucker multipliers.

The first-order and complementary slackness conditions are:

$$u_{c_1} - \mu_1 = 0,$$

$$u_{l_1} - \mu_1 w = 0,$$

$$\delta u_{c_2} - \mu_2 = 0,$$

$$\delta u_{l_2} - \mu_2 w = 0,$$

$$\delta^2 u_{c_3} - \mu_3 = 0,$$

$$\mu_1 - (1+r)\mu_2 - \mu_4 = 0,$$

$$\mu_2 - (1+r)\mu_3 - \mu_5 = 0,$$

$$\mu_4[E_1 - a(1 + r_H)H_0 + \pi H_0] = 0,$$

$$E_1 \leq a(1 + r_H)H_0 - \pi H_0,$$

$$\mu_4 \geq 0,$$

$$\mu_5[E_2 - a(1 + r_H)^2 H_0 + \pi H_0] = 0,$$

$$E_2 \leq a(1 + r_H)^2 H_0 - \pi H_0,$$

$$\mu_5 \geq 0.$$

These first order conditions (FOCs) imply that, the optimum is characterized by equal marginal utility of consumption and labor within as well as between periods. The FOCs also imply that the following hold:

$$u_{c_1} = u_{l_1}/w = (1 + r)\delta u_{c_2} + \mu_4 = (1 + r)\delta u_{l_2}/w + \mu_4 \tag{A1}$$

$$\delta u_{c_2} = \delta u_{l_2}/w = (1 + r)\delta^2 u_{c_3} + \mu_5, \tag{A2}$$

where, μ_4 and μ_5 are the multipliers on the collateral constraints in period 1 and 2, respectively. Let l_t^C denote period t leisure when the collateral constraints bind ($\mu_4 > 0, \mu_5 > 0$) and l_t^{NC} when they do not bind ($\mu_4 = 0, \mu_5 = 0$). Assuming separability in consumption and leisure and using analysis similar to Rossi and Trucchi (2016), equation (1) implies that $u_{l_1}^C > u_{l_1}^{NC}$ and, therefore intuitively, $l_1^C < l_1^{NC}$, i.e., when the collateral constraint binds, leisure is lower and labor supply higher. Unlike period 1, such informal analysis of FOCs reveals no clear relationship between the constraints and labor supply in period 2—(1) suggests that $l_2^C > l_2^{NC}$, (2) implies that $l_2^C < l_2^{NC}$.

For the special case of households facing binding collateral constraints, further insights can be gained by assuming an intertemporally separable log utility function that is also separable in consumption and leisure. In this case, the optimal solutions for leisure in period 1 and 2 are:

$$l_1^* = \frac{w + a(1 + r_H)H_0 - (1 + r)\pi H_0 - A_1}{2w}$$

$$l_2^* = \frac{w + a(r_H - r)(1 + r_H)H_0 + (1 + r)A_1 - A_2}{2w}$$

Note that l_1^* varies positively with ease of credit access, a, if home value, $(1 + r_H)H_0$, is positive. So as a increases and the collateral constraint becomes less binding, leisure increases and labor supply declines in period 1. However, the relationship between a and l_2^* remains ambiguous, as it depends on the sign of $(r_H - r)$.[27]

Comparative Statics

Now let us do comparative statics of the optimal choice l^* with respect to a using these conditions, i.e., let us derive dl_1^*/da. First, note that a only directly determines the first-period credit constraint on E_1. If the first-period collateral constraint does not bind, $\mu_4 > 0$, $E_1^* < a(1 + r_H)H_0 - \pi H_0$, and $dE_1^*/da = 0$. On the other hand, if the first-period collateral constraint binds, $\mu_4 = 0$, $E_1^* = a(1 + r_H)H_0 - \pi H_0$, and $dE_1^*/da = (1 + r_H)H_0 > 0$. Putting the two cases together, we know that:

$$\frac{dE_1^*}{da} \geq 0.$$

By the chain rule and making use of the previous equation yields the following sign of dl_1^*/da up to weak inequality:

$$sign\left[\frac{dl_1^*}{da}\right] = sign\left[\frac{dl_1^*}{dE_1^*}\frac{dE_1^*}{da}\right] = sign\left[\frac{dl_1^*}{dE_1^*}\right].$$

[27]Although we don't formally model present-biased preferences, it is worth noting that the existence of present-bias also would reinforce the notion that relaxing collateral constraints should lower labor supply in the first period and have ambiguous effects in the second period. Previous research on present-biased preferences has shown that, in a setting without home equity, impatience leads to lower lifetime consumption and labor supply, as well as a shift of future consumption toward the present (Laibson, 1997; Fredrick, Loewenstein, and O'Donoghue, 2002; O'Donoghue and Rabin, 1999). With home equity extraction, present-biased preferences should amplify a home-equity financed consumption shift to period 1 from the future. This leads to a larger first-period labor supply decline. The effect on second-period labor supply should be more ambiguous than without present-biased preferences. While impatience lowers second-period labor supply by increasing the home-equity-financed consumption transfer from period 3 to period 2, higher debt servicing requirements due to higher first-period home equity withdrawal should have an offsetting effect.

For comparative statics of l_1^* with respect to E_1^*, first plug in the budget constraint into the first-period FOCs:

$$u_c[w(1-l_1) - r\pi H_0 + E_1 - A_1, l_1] = \frac{u_l[w(1-l_1) - r\pi H_0 + E_1 - A_1, l_1]}{w}.$$

Then, differentiation with respect to E_1 yields:

$$u_{c_1c_1}\left(-w\frac{dl_1}{dE_1}+1\right) + u_{c_1l_1}\frac{dl_1}{dE_1} = \frac{1}{w}\left[u_{c_1l_1}\left(-w\frac{dl_1}{dE_1}+1\right) + u_{l_1l_1}\frac{dl_1}{dE_1}\right],$$

$$\frac{dl_1^*}{dE_1} = \frac{-wu_{c_1c_1} + u_{c_1l_1}}{-w^2u_{c_1c_1} - u_{l_1l_1} + 2wu_{c_1l_1}} \lesseqgtr 0.$$

Combining this equation and the previously derived sign condition for dl_1^*/da, we see that the sign of dl_1^*/da is ambiguous with, as we write in the main text:

$$sign\left[\frac{dl_1^*}{da}\right] = sign\left[\frac{dl_1^*}{dE_1^*}\right] = sign\left[\frac{-wu_{c_1c_1} + u_{c_1l_1}}{-w^2u_{c_1c_1} - u_{l_1l_1} + 2wu_{c_1l_1}}\right].$$

Similarly, we can derive the equation for the sign of dc_1^*/da.

On the other hand, if utility is non-separable in c and l, then even the unambiguous effect of easier credit access on labor supply in period 1 disappears. In this case, based on the system of FOCs, comparative statics of c_1^* and l_1^* with respect to a, yield:

$$sign\left[\frac{dc_1^*}{da}\right] = sign\left[\frac{-u_{l_1l_1} + wu_{c_1l_1}}{-w^2u_{c_1c_1} - u_{l_1l_1} + 2wu_{c_1l_1}}\right],$$

$$sign\left[\frac{dl_1^*}{da}\right] = sign\left[\frac{-wu_{c_1c_1} + u_{c_1l_1}}{-w^2u_{c_1c_1} - u_{l_1l_1} + 2wu_{c_1l_1}}\right].$$

Assuming convex preferences with diminishing marginal utility of consumption and leisure ($u_{cc} \leq 0$ and $u_{ll} \leq 0$), the direction of the effect of a is ambiguous and depends on the magnitude of the cross derivatives relative to the second order derivatives. In the special case with utility separable in consumption and leisure ($u_{cl} = 0$), improved credit access unambiguously (weakly) increases consumption and leisure in period 1, and hence lowers labor supply.

Thus, the effect of credit access on consumption and leisure in period 1 is analogous to the income effect in standard labor supply models; preferences separable in c and l imply that both are normal goods and, therefore, improved credit access has positive income effects. However, if consumption and leisure are non-separable ($u_{cl} < 0$), the theoretical prediction of the effects of improved credit access could be ambiguous.