Labor Market Effects of Credit Constraints: Evidence from a Natural Experiment

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Abstract

We exploit the 1998 and 2003 constitutional amendment 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 county-level data and the synthetic control approach, we find that easier access to housing credit led to a notably lower labor force participation rate between 1998 and 2007. We show that our findings are remarkably robust to improved synthetic control methods based on insights from machine-learning. We explore treatment effect heterogeneity using grouped data from the basic monthly CPS and find that declines in the labor force participation rate were larger among females, prime age individuals, and the college-educated. Analysis of March CPS data confirms that the negative effect of easier home equity access on labor force participation was largely concentrated among homeowners, with little discernible impact on renters. We find that, while the labor force participation rate experienced persistent declines following the amendments that allowed access to home equity, the impact on GDP growth was relatively muted. Our research shows that labor market effects of easier credit access should be an important factor when assessing its stimulative impact on overall growth.

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1. Introduction

Prevalence of household credit constraints can pose major challenges to the pace of economic activity. Thus, easing such constraints and facilitating improved access to credit remains a key public policy objective during economic slowdowns. Easier credit access can boost the economy through consumer spending, as borrowing is an important vehicle of consumption smoothing (Mian and Sufi, 2011). But when credit is tight, households can alternatively smooth consumption by increasing labor supply. Therefore, the net effect of easier credit access on economic activity depends not only on its impact on consumer spending but also on its effect on labor supply. While a large body of research has examined the effect of credit constraints on consumer spending and saving, most assumed labor supply to be fixed (Athreya, 2008). Just a handful of recent papers directly examined the impact of credit constraints on labor supply.

Using a standard life-cycle model of consumption and labor supply and data from the Italian Survey of Households Income and Wealth (SHIW), Rossi and Trucchi (2016) found that men facing binding liquidity constraints worked on average 4 hours more. More recently, using staggered passage of branch-banking deregulation laws across U.S. states, Bui and Ume (2016) found that, although weekly hours declined by 0.5 following branch-banking deregulation, the effect on the extensive margin (i.e. labor force participation) was insignificant.[1] To the best of our knowledge, there exists no formal investigation of the effects on the U.S. labor market of policies specifically restricting access to home equity borrowing—by far the dominant source of credit for a vast majority of American households.

[1]Among somewhat older papers on credit constraint's effect on the labor market, see Worswick (1999) and Del Boca and Lusardi (2003). A related strand of the literature found positive effects of mortgage debt on labor supply, but did not focus on credit constraints, per se. For other related research, see a brief literature review in section 2.

We extend the research on labor supply effects of credit constraints by exploiting the 1998 and 2003 constitutional amendments in Texas—allowing access to closed-end home equity loans and lines of credit for non-housing purposes—as natural experiments and make three contributions. First, to the best of our knowledge, we are the first to estimate the labor market effects of such a large and plausibly exogenous shock to home equity borrowing constraints in the U.S. In so doing, we focus on a broad measure of the state of the labor market—the labor force participation rate (LFPR). Secondly, we extend the basic two-period theoretical model of Rossi and Trucchi (2016) to a three-period setting with collateral constraints. We then 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. And finally, using the synthetic control methodology and its recent refinements based on insights from machine learning, we shed light on the overall effect of the constitutional amendments introducing home equity lending to Texas, not only on the LFPR, but also on GDP growth.

By focusing on labor market effects, the paper complements a small set of recent papers that have also exploited the Texas amendment as a source of exogenous shocks for outcomes other than labor supply. Most notably, Abdallah and Lastrapes (2012) used the Texas amendment as a source of exogenous variation in credit constraints to provide compelling evidence that increased access to home equity borrowing spurred consumer spending.[2] More recently, Zevelev (2016) showed that by removing restrictions on home equity borrowing, the Texas amendment contributed to a 3 to 5 percent increase in house prices over the 6 years following the law change.[3] But the labor market effects of the amendment in Texas remain still unexplored.

[2] Leth-Petersen (2010) and Agarwal and Qian (2017) used home equity borrowing reforms in Denmark and Singapore, respectively, to estimate their impact on consumer spending.
[3] Stolper (2014) found that a 2003 law that opened up Home Equity Lines of Credit (HELOC) in Texas led to gains in access to higher education financed by home equity borrowing. Kumar (2018) shows that restricted access to home

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. Access to home equity should clearly have meant more to homeowners than renters, who did not have home equity. Therefore, strikingly different trends in the LFPR after 1998 for homeowners (Appendix Figure A1) and renters (Appendix Figure A2) in Texas vs. other states further reinforce the view that home equity access could have led to the decline in the LFPR for homeowners in Texas relative to other states.

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 state-level policies that may have changed concomitantly and affected Texas differently than other states. For example, the period surrounding the Texas amendment saw sharp swings in oil prices (Appendix Figure A3), 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 county-level data, we find that, by opening the home equity lending market to Texas' homeowners, the 1998 and 2003 amendments led to persistent declines in the LFPR between 1998 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

equity borrowing that limited excessive leverage during the housing boom in Texas relative to the nation lowered mortgage default rates.

control methods that account for the potential violation of the common trends assumption. We proceed by optimally weighting comparison states to construct a synthetic control group 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 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 controls 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. (2017).

Our preferred estimates suggest that access to home equity loans led to about a 1 percent average decline in the LFPR in the first 5 years between 1998 and 2002—an effect that subsided after 2001, but almost doubled between 2004 and 2007, after Home Equity Lines of Credit (HELOCs) became available. We find that easier access to home equity led to a 1.3 percentage point average decline in LFPR over 10 years. 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, prime-age population, and the college-educated. Finally, we use grouped data from the March CPS to find that there was a significant decline in the LFPR of homeowners, but little discernible effect on renters—a group not directly affected by the law change.[4] Our findings are different from previous work that found labor supply effects of credit constraints mainly on the intensive margin.

[4] See Flood, King, Ruggles, & Warren (2015) for details on IPUMS-CPS.

While the Texas amendment spurred consumer spending (Abdallah and Lastrapes, 2012) and supported house price growth (Zevelev, 2016), our estimates suggest it also reduced the LFPR, eroding gains from easier credit access to the Texas' economy. We confirm this intuition and find that easier access to home equity did not affect real GDP growth in Texas.[5] Our estimates have 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 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. Our research also has implications for the labor market effects of easing restrictions on other forms of borrowing against current wealth—for example 401(k) accounts.

The rest of the paper is organized as follows. Section 2 presents a brief review of the previous literature on the labor market effects of credit constraints. Section 3 presents the theoretical framework. Section 4 discusses the Texas 1997 amendment allowing home equity access and section 5 describes the data. Econometric specifications and estimation results are discussed in section 6, and section 7 concludes.

2. Previous Literature

Using a standard life-cycle model of consumption and labor supply, Rossi and Trucchi (2016) showed that liquidity constraints negatively affect labor supply. They used data from the Italian Survey of Households Income and Wealth (SHIW) and found that men facing binding

[5] While this may appear somewhat counter-intuitive at first, it is consistent with Jappelli and Pagano (1994), who showed that liquidity constraints may positively affect growth.

liquidity constraints—those with current income below their permanent income—worked on average 4 hours more. Lacking an exogenous shock to liquidity constraints through clear change in policy, Rossi and Trucchi (2016) relied on fixed effects and plausible instrumental variables to deal with endogeneity. More recently, using the staggered passage of branch banking deregulation laws across U.S. states, Bui and Ume (2016) found that, although weekly hours declined by 0.5 after bank branching deregulation eased credit access, the effect on the extensive margin (i.e. labor force participation) was insignificant.[6] Using a structural model of intertemporal labor supply and data from the Canadian census, Worswick (1999) found that, immigrant households were more likely to be credit-constrained during the first few years of their arrival in Canada and, therefore, immigrant wives worked longer hours to support family consumption. While a positive relationship between credit constraints and labor supply found in these three papers is consistent with the standard life-cycle model's prediction that credit-constrained households can smooth consumption by increasing labor supply, it is also the case that higher debt due to easier credit access would add to the household's debt service commitments, requiring them to work more.

Del Boca and Lusardi (2003) used SHIW data from 1989–93 to estimate the effect of easier availability of mortgages on LFP using plausibly exogenous variation from entry of foreign banks and new banking legislation and found that, even as credit access increased through easier mortgage availability, adding mortgage obligations to household debt positively affected wives' LFP.

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;

[6] Using PSID from 1967-1970, Dau-Schmidt (1997) found that liquidity bound primary male workers (those with zero liquid assets) have 1% lower intertemporal labor supply response.

Bottazzi, 2004; Butricia and Karamcheva, 2013; Lusardi and Mitchell, 2017; Maroto, 2011; Houdre, 2009; Cao 2017). But the evidence of a positive relationship between mortgage debt and labor supply remains far from conclusive. Using British Household Panel Survey data from 2001–06, Pizzinelli (2017) found that wives' labor supply was negatively related with loan-to-value (LTV) ratio, but positively related with husbands' loan-to-income (LTI) ratio. High-LTV households' behavior appears more elastic on the extensive margin than low-LTV households. Bernstein (2015) also found negative effects of being underwater on household labor supply.[7]

As is clear from this brief review, with the exception of Bui and Ume (2016), the previous research generally lacked a clearly exogenous shock to credit constraints in order to disentangle the aggregate impact of easier credit access on the labor market from other potentially confounding macroeconomic shocks. Such a gap is particularly striking in research on labor market effects of home equity borrowing constraints, where previous work focused almost exclusively on consumer spending (e.g. Abdallah and Lastrapes, 2012); Leth-Petersen, 2010). Our paper fills this void by estimating the labor market effects of easier access to home equity credit using plausibly exogenous variation from the natural experiment in Texas, which for the first time in the state's history allowed home equity loans for non-housing purposes.

3. Theoretical Framework

[7]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 (Atalay, Barrett, & Edwards, 2016; Disney and Gathergood, 2013; Milosch, 2014; Fu, Liao, & Zhang, 2016; Bottazzi, Trucchi, & Wakefield, 2017; Zhao and Burge, 2017). But a consensus on the effect of house price growth on labor supply remains elusive. Estimating heterogeneous effects, He (2015) found that younger age groups, being short on housing, increased LFP in response to an increase in house prices. For older households, however, a potential negative wealth effect on LFP was more than offset by a positive bequest motive. Yoshikawa and Ohtake (1989) also found a positive effect of an increase in house prices on married female's LFP. Adding to the mixed evidence that exists in this literature, Johnson (2014) found little evidence of a positive effect of house prices on married women's labor force participation but positive effect on female earnings.

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 $E_2(1+r)$.

The first order conditions (FOCs) derived in Appendix B 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:

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

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

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 $(r_H - r)$.[8]

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 in Appendix B, comparative statics of c_1^* and l_1^* with respect to a, yield:

$$sign\left[\frac{dc_1^*}{da}\right] = sign\left[\frac{-u_{l_1 l_1} + wu_{c_1 l_1}}{-w^2 u_{c_1 c_1} - u_{l_1 l_1} + 2wu_{c_1 l_1}}\right],$$

$$sign\left[\frac{dl_1^*}{da}\right] = sign\left[\frac{-wu_{c_1 c_1} + u_{c_1 l_1}}{-w^2 u_{c_1 c_1} - u_{l_1 l_1} + 2wu_{c_1 l_1}}\right].$$

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.

[8] 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.

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.

4. Texas 1997 Home Equity Amendment

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 home purchase, the Texas constitution allowed using the home as collateral primarily for just two other purposes: (1) home improvements and (2) taxes (Graham, 2007). Almost all other forms of home equity borrowing remained out of bounds for Texas homeowners.[9] 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.[10] The amendment took effect on January 1, 1998. Although total

[9] 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), Zevelev (2016), and Kumar and Skelton (2013).

[10] 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

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

5. 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. While we extend the pre-treatment period back to 1980 to explore robustness of our estimates to richer specifications and improved methodologies, starting with 1992 helps us avoid differential trends in Texas vs. other states due the 1980's recessions, the saving and loans crisis, and the 1991 recession. 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). We use average hourly earnings of

amendment also prohibited home equity loans with balloon payments, negative amortization, and pre-payment penalties. Further, HELOCS remained prohibited until 2003.

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. 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.

We also test the robustness of our state-level estimates to use of county-level data. The county-level LFPR is calculated as the county-level size of the labor force divided by county-level population age 16 and older. Appendix Table A1 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. Because basic monthly CPS lacks information on homeownership, we use March supplements of the IPUMS-CPS to examine differences in estimated effects for homeowners vs. renters.

6. Econometric Specification and Estimation Results

6.1 Difference-in-Differences Specifications

Using state-level data to estimate the effect of the Texas' 1998 amendment, our benchmark difference-in-differences (DID) specification with state and time-fixed effects is as follows:

$$Y_{st} = \beta^{HEL} D_s^{TX} \times D_t^{Post-HEL} + \beta^{HELOC} D_s^{TX} \times D_t^{Post-HELOC} + \boldsymbol{X}_{st}\gamma + \delta_t + \alpha_s + \eta_{st}, \qquad (3)$$

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 the 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.[11]

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 other macroeconomic shocks common to the state as well as to the nation. The DID identifying assumption is that state-by-time effects, η_{st}, are random and uncorrelated with the policy variables ($D_s^{TX} \times D_t^{Post-HEL}$ and $D_s^{TX} \times D_t^{Post-HELOC}$) i.e., $E[\,\eta_{st} | D_s^{TX} \times D_t^{Post}, \boldsymbol{X}_{st}] = 0$. 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 pre-treatment-path for the remaining states can serve as valid counterfactuals for Texas' LFPR in the post-treatment period.

Panel A of Table 1 reports results for the conventional DID specification in (3). Column (1) shows estimates from the DID model with just state and time-fixed effects, without other

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

covariates. Relative to the pre-treatment period (1992–97), 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 impact of 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.[12]

The DID estimates subside in column (2), that adds key state-level economic covariates consistent with theory and state-level demographic covariates.[13] Like column (2), Conley-Taber confidence intervals suggest that $\hat{\beta}^{HEL}$ remains imprecisely estimated. To account for region-specific macro shocks, Column (3) includes census division-by-year effects and column (4) adds state-specific linear time trends. They show that access to home equity lowered the LFPR in Texas and that the effect of HELOC after 2003 (-1.3 to -1.6 percentage points) exceeded that of the HEL after 1997 (-0.4 to -0.5 percentage points). Conley-Taber confidence intervals continue to reflect statistical significance of $\hat{\beta}^{HELOC}$, although $\hat{\beta}^{HEL}$ remains noisy.

The sensitivity of DID estimates across specifications in Panel A suggests that controlling for state-specific macro shocks remains a formidable challenge, particularly because Texas reacts differently to swings in oil prices. To ease this concern, 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 (1992–97). The DID estimates are qualitatively similar to those in columns (3) and (4) of 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.

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[12] 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.

[13] 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 high school, 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 2. For the model with covariates and census division-by-year effects in Panel A, the DID estimates are larger for females than males, for the prime-age group relative to the 55+, and for the college-educated compared with those lacking college education.

The difference by gender is consistent with the previous labor supply literature that found that females are more elastic than males, particularly on the participation margin. Credit constraints are likely to be more binding on the prime-age group relative to older individuals. Larger effect for the college-educated is somewhat surprising given that they are less credit-constrained, but could stem from their higher homeownership rate and borrowing ability.

The DID estimates in Panel B, although qualitatively similar, are somewhat more imprecise and reflect considerable sensitivity to inclusion of state-specific time trends. This warrants a more careful examination of the key identification assumption—that the LFPR trends between Texas and control states be parallel in the absence of intervention. An informal test is to rule out pre-existing trends using a fully time-varying specification.

Time-varying DID Estimates

Letting the DID coefficient be time-varying, we estimate the following specification to explore the dynamic effects of access to home equity:

$$Y_{st} = \sum_{t<1997} \beta_t \, D_s^{TX} \times D_t + \sum_{t>1997} \beta_t \, D_s^{TX} \times D_t + \boldsymbol{X}_{st}\gamma + \delta_t + \alpha_s + \eta_{st}, \qquad (4)$$

where D_t denotes an indicator variable for year t. We treat 1997—the year just before the policy change—as the base year, so that β_t can be interpreted as the effect of the amendment relative to year 1997. Due to space constraints, we report time-varying DID coefficients on $D_s^{TX} \times D_t$ in Appendix Table A2 (using state-level data) and Appendix Table A3 using (county-level data).

If DID assumptions hold, then we should see insignificant coefficients on $D_s^{TX} \times D_t$ interactions before the law change in 1998. Plotting time-varying DID coefficients from column (3) of Appendix Table A2 for the fixed-effects specification with covariates and division-by-year effects, Appendix Figure A4 shows evidence broadly consistent with the DID assumptions—estimates *before* 1997 are not statistically different from those in 1997. However, in the years *after* the law change, they are mostly negative and statistically different from zero. Analogous estimates using county-level data plotted in Appendix Figure A5 display a similar pattern, but exhibit some evidence of pre-existing trends in outcomes.

Summary of DID Results

Conventional DID estimates reported in Tables 1 and 2 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. Time-varying DID estimates also show that the estimated effect weakened significantly 3 years after the HEL access was allowed, but strengthened in the post-HELOC period.[14]

Nevertheless, the relative fragility of DID estimates to state-specific time trends remains a key concern, and it is particularly striking in Appendix Tables A2 and A3. Time-varying estimates in models with state-specific time trends in column (4) are too noisy to infer anything about LFPR

[14] In results not presented due to space constraints, we examined the robustness of the estimates from fixed effects specifications presented in Appendix Figure A4 to first-differenced specifications, specifications with lagged dependent variable using Arellano and Bond's dynamic panel data model. The results were qualitatively similar. Time-varying DID estimates using county-level data presented in Appendix Table A3 are also qualitatively similar to state-level estimates presented in Appendix Table A2.

reaction to home equity access in Texas. Such sensitivity to state-specific trends can have two alternative interpretations.

First, in addition to imprecision stemming from the loss of several degrees of freedom, 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.

The other interpretation is 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 for the treated state. To circumvent this, we next use the synthetic control method of Abadie et al. (2010) (henceforth SCM-ADH).

6.2 Synthetic Control Estimates

Unlike DID, which requires time-constant state effects (α_s), the SCM-ADH estimator allows those to be time-varying. The no-treatment counterfactual follows an unobserved common factor model:

$$Y_{st}^{N} = \boldsymbol{X}_{st}\gamma_t + \delta_t + \mu_t\alpha_s + \eta_{st}, \tag{5}$$

where μ_t are common factors and α_s their loadings. Let $t = 1 \ldots T_0$ denote the pre-treatment period and $t = T_0 + 1 \ldots T$ the post-treatment. 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".

$$\hat{\beta}_{TX}^{t} = Y_{TXt} - \hat{Y}_{TXt}^{N} = Y_{TXt} - \sum_{s \neq TX} w_s\, Y_{st} \tag{6}$$

Subject to standard SCM-ADH assumptions, Texas minus "synthetic Texas" gap for $t > T_0$, $\hat{\beta}_{\text{TX}}^{\text{t,Post}}$, yields an unbiased estimates of 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.[15]

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. This leads to the constrained regression model discussed in Doudchenko and Imbens (2016).

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, $\hat{\beta}_{\text{TX}}^{\text{t,Post}}$, column (1) of Table 3 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 ($\hat{\mathbf{W}}$) for control states are reported in Appendix Figure A6.

[15] $\left\|\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W}\right\| = \sqrt{(\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W})'\mathbf{V}(\mathbf{Z}_{\mathbf{TX}}^{\mathbf{Pre}} - \mathbf{Z}_{\mathbf{-TX}}^{\mathbf{Pre}}\mathbf{W})}$, 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 et al. 2014; Galiani and Quistorff, 2016).

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 and may be too conservative (Abadie et al., 2015; Cavallo Galiani, Noy, & Pantano, 2013). Using pre-treatment Root Mean Squared Prediction Error ($\mathrm{RMSPE}^{\mathrm{Pre}}$)—calculated as $\sqrt{1/T_0 \sum_{t \leq T_0} \left(\hat{\beta}^{\mathrm{t,Pre}}\right)^2}$—as a measure of match quality, one solution is to conduct inference based on standardized 2-sided p-values:

$$\text{P-value}_t^{\mathrm{std}} = \Pr\left(\frac{\left|\hat{\beta}_{\mathrm{PL}}^{\mathrm{t,Post}}\right|}{\mathrm{RMSPE}_{\mathrm{PL}}^{\mathrm{Pre}}} \geq \frac{\left|\hat{\beta}_{\mathrm{TX}}^{\mathrm{t,Post}}\right|}{\mathrm{RMSPE}_{\mathrm{TX}}^{\mathrm{Pre}}}\right) \quad (7)$$

Standardized p-values reported in square brackets in column (1) of Table 3 suggest that standardized $\left|\hat{\beta}_{\mathrm{TX}}^{\mathrm{Post}}\right|$ 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, $\overline{\hat{\beta}_{\mathrm{TX}}^{\mathrm{Post}}}$, reported in the bottom panel of Table 3, also is an extreme outlier among all states.[16] In contrast, the p-value calculated similarly for the pre-treatment average effect, $\overline{\hat{\beta}_{\mathrm{TX}}^{\mathrm{Pre}}}$, equals 1, suggesting that the pre-treatment difference in outcomes between Texas and its counterfactual is not significantly different from those for other states.

[16] Standardized p-value for $\overline{\hat{\beta}_{\mathrm{TX}}^{\mathrm{Post}}}$ are based on $\frac{\mathrm{RMSPE}_{\mathrm{TX}}^{\mathrm{Post}}}{\mathrm{RMSPE}_{\mathrm{TX}}^{\mathrm{Pre}}}$, where $\mathrm{RMSPE}^{\mathrm{Post}} = \sqrt{\frac{1}{T-T_0} \sum_{T_0+1 \leq t \leq T} \left(\hat{\beta}^{\mathrm{t,Post}}\right)^2}$. Appendix Figure A7 plots the normalized average post-RMSE for Texas along with that of other states and shows that Texas is an extreme outlier.

To get a sense of the treatment effect for HELOC, separately from HEL, Figure 3A and 3B 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.[17]

To address concerns that SCM-ADH specifications based on all pre-treatment lags may be subject to overfitting, column (2) of Table 3 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 house price. Estimated treatment effects are larger than those 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. The overall pattern of estimated treatment effects plotted in Appendix Figure A10 is again qualitatively similar to those in column (1) and the placebo estimates presented in Appendix Figure A11 show that $\hat{\beta}_{\mathrm{TX}}^{\mathrm{t,Post}}$ are unusually negative.

Robustness to Alternative Donor Pools

Column (3) of Table 3 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-

[17] Analogous to Appendix Figures A6 and A7, Appendix Figures A8 and A9 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.

value of 0.09. We also considered alternative donor pools, limiting them 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.[18] Figure 4A shows that the estimated treatment effects are qualitatively similar across alternative donor pools.

Treatment Effect Heterogeneity

The last two columns of Table 3 report SCM-ADH estimates for employment rate among homeowners in column (4) and renters in column (5), using aggregate data from the March CPS, which has information on homeownership. While the temporal pattern of treatment effect for homeowners in column (4) is similar to those in columns (1)-(3), it is different for those not owning homes in column (5). Appendix Figure A12 plots estimates reported in column (4) and column (5) and shows that the effects for homeowners are consistently negative, but those for renters fluctuated with no clear pattern; the average post-treatment average effect for homeowners was -1.4 percentage points, substantially larger than just -0.2 percentage points for the renters.

Finally in Figure 4B we examine heterogeneity in SCM-ADH estimates across demographic groups and show that the estimated treatment effect drifted in the negative territory for almost all demographic groups, with the effects generally larger for females than males, for the

[18] 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 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.

prime-age group relative to the 55+, and for the college-educated compared with those without college education.

6.3 SCM 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:

$$\hat{Y}_{TXT}^{N} = \kappa + \sum w_i Y_{iT} \qquad (8)$$

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 ($w_i = \overline{w}$), 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.[19]

[19] For example, Doudchenko and Imbens (2016) noted that the no intercept restriction implies absence of any permanent differences between the treated group and the controls; 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

Model 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. Letting $\mathbf{Y}_{\mathbf{TX}}^{\mathbf{pre}}$ denote the vector of pre-treatment outcomes for the treated unit (Texas), $\boldsymbol{\kappa}$ the intercept, $\mathbf{Y}_{-\mathbf{TX}}^{\mathbf{pre}}$ the matrix of pre-treatment outcomes for the control units, and $\mathbf{W}$ a conformable state-specific vector of weights, the model with elastic net penalty (henceforth SCM-Elastic Net) can be written as:

$$\left\|\mathbf{Y}_{\mathbf{TX}}^{\mathbf{pre}} - \boldsymbol{\kappa} - \mathbf{Y}_{-\mathbf{TX}}^{\mathbf{pre}}\mathbf{W}\right\| + \lambda\left(\frac{1-\alpha}{2}\sum_{i=1}^{N}|w_i| + \alpha\sum_{i=1}^{N}w_i^2\right) \qquad (9)$$

Over a grid of values for the tuning parameters (α and λ), the optimal combination of α and λ is chosen to minimize the average of out-of-sample RMSPE across all control states, by estimating the model over a training sample and calculating the RMSPE over a test sample for each control state as a pseudo-treated unit. The training and test samples for each control state are formed by splitting the pre-treatment sample into roughly two equal parts.

Matrix Completion Approach

In another recent paper, Athey et al. (2017) 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 ($\boldsymbol{Y}$) by minimizing a convex function of the difference between the observed matrix and the unknown complete matrix using nuclear norm regularization. Letting

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pre-treatment periods is significantly smaller than the number of units, requiring alternative procedures to select among the set of estimated weights.

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:

$$\widehat{\boldsymbol{Z}} = \arg\min_{\boldsymbol{Z}} \sum_{(i,t)\in\Omega} \frac{(Y_{it} - Z_{it})^2}{|\Omega|} + \lambda\|Z\|_*, \qquad (10)$$

where $\|Z\|_*$ is the nuclear norm (sum of singular values of $\boldsymbol{Z}$**).**[20] The regularization parameter, λ, is chosen using five-fold cross-validation. Athey et al. (2017) show that solving for the missing counterfactual using this matrix completion problem exploits richer patterns in the data and using extensive simulations 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

To implement the two new approaches and compare the results with DID and SCM-ADH, we extend the pre-treatment period back to 1980 for two reasons. First, it allows us to evaluate how the results change relative to the shorter pre-treatment window used earlier in the paper. And secondly, an 18-year pre-treatment window better meets the requirement in SCM models that the number of pre-intervention periods be large. Table 4 summarizes the main results, and estimates plotted in Figure 5 show that their overall temporal pattern is qualitatively similar to that from the traditional SCM-ADH approach, though there are subtle differences. Particularly striking is that, as suspected earlier, the equal weighting of control states in the DID model is unable to generate

[20] 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.

parallel trends between Texas and the control states and, therefore, 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", except for a brief period surrounding the 1991 recession; MC-NNM appears to perform the best. Appendix Figures A13, A14, and A15 plot the estimated effects for Texas together with those for the remaining states as placebos and confirm that, while all three approaches yield largely similar patterns post-treatment, MC-NNM appears to generate the closest counterfactuals.[21]

Pre-treatment RMSPEs reported in the bottom panel of Table 4 suggest that MC-NNM by far has the lowest RMSPE for Texas (0.07) as well as the remainder of control states (0.1). The average treatment effect of a 1.3 percentage point decline in LFPR is very similar to that from SCM-Elastic Net, although substantially larger than the 1 percentage point effect from SCM-ADH. Standardized p-values are generally larger than those for the baseline SCM-ADH models reported earlier, but estimates turn significant for periods after 2003. The p-value of 0.08 for the average effect over 10 years post-treatment indicates that the impact of credit access was significant at the 10 percent level. Appendix Figure A16 plots the empirical CDF of MC-NNM estimates of 10-year average effects across states and shows that the -1.3 percentage point estimate for Texas clearly stands out in the lower tail of that distribution.

Impact on GDP Growth

At the outset, we surmised that a potential negative effect of easier credit access on LFPR should damp its stimulative effect on the overall economy. In Table 5, we show that the amendment

[21] Figure A17 plots SCM-ADH estimates along with DID, SCM-Elastic Net, and MC-NNM for the donor pool of energy states and shows that the overall pattern and magnitude of estimated effects are very similar to Figure 5 for the all states sample. Placebo estimates corresponding to the MC-NNM estimates are plotted in Figure A18 and show that Texas' MC-NNM estimates are is in the bottom tail among energy states.

allowing access to home equity borrowing in Texas had a relatively small and insignificant impact on real GDP growth. Table 5 is isomorphic to Table 4; it differs only in reporting results for the annual real GDP growth as the outcome variable instead of the LFPR. Unlike results for the LFPR in Table 4, Table 5 shows no clear pattern of an effect on real GDP growth in Texas relative to "synthetic Texas".

Standardized p-values reflect statistical insignificance. The bottom panel suggests that the post-treatment average effect differs widely across the four models. P-values for the significance of the average post-treatment effect are close to 1. The best-performing model is SCM-Elastic Net followed by MC-NNM, and both suggest that the impact was negative and insignificant. Figure 6 plots the estimated dynamic effects for the four models and, unlike Figure 5 for the LFPR, reveals no clear evidence of an impact on real GDP growth. We conclude that the amendment allowing easier access to HEL, which lowered LFPR by 1.3 percentage points, had a minimal impact on real GDP growth.

7. Conclusion

We use a 1997 constitutional amendment that allowed access to home equity loans in Texas as a natural experiment to estimate the effect of easier credit access on the labor market. Using aggregate state- and county-level data, we find that easier access to housing credit led to a notable decline in the LFPR between 1998 and 2007. Analysis of March CPS data confirms that the negative effect of easier home equity access on labor force participation was concentrated among homeowners, with little impact on renters—a group not directly affected by the reform. Employing the synthetic control approach and its recent refinements based on insights from machine learning, we find that the LFPR persistently declined following the amendment allowing home equity loans, while real GDP growth remained largely unaffected. Our preferred estimates suggest that easier access to home equity led to a -1.3 percentage point decline in the LFPR, on average, over 10 years. A key policy implication is that labor market effects of easier credit access should be an important factor when assessing its stimulative impact on overall growth.

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 other states responded differently to those changes than Texas, and if the timing of those responses were concomitant with the onset of easier home equity access, our estimates may be biased. Likely differential impact of changes in oil prices on Texas vs. the rest of the nation is also a potential concern, although our estimates are robust to restricting the analysis to the subsample of energy-intensive states.

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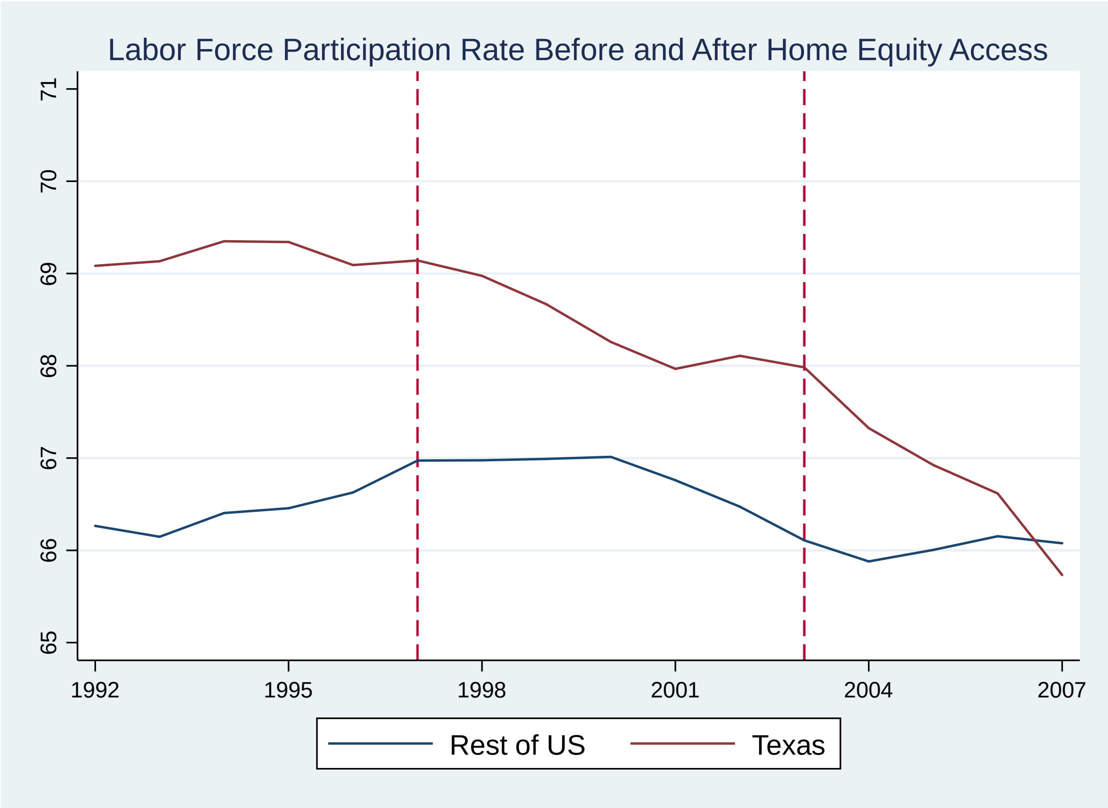
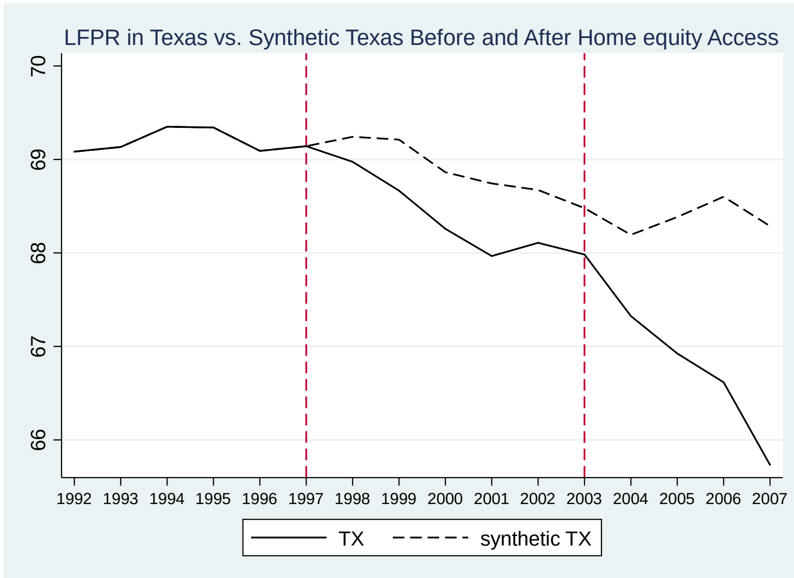
Figure 1



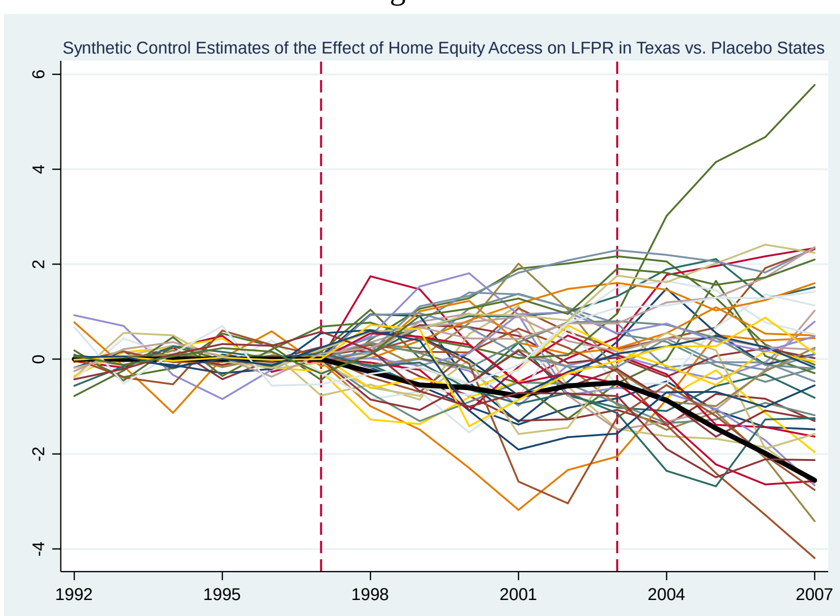
Notes: Using data from BLS-LAUS program, the figure plots state-level LFPR for Texas and the weighted-average LFPR (weighted by population) for the remaining states. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. Sources: BLS/LAUS; Authors' calculations.

Figure 2A



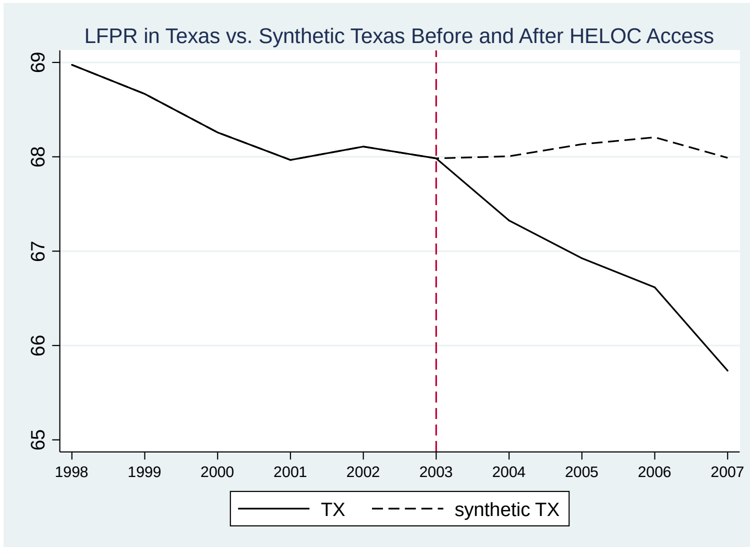
Notes: The figure shows the pre-HEL (1992-1997) and post-HEL (1998-2007) LFPR path for the treatment group (Texas) and the weighted average of control states (synthetic-Texas) using the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for 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, 2016). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 2B



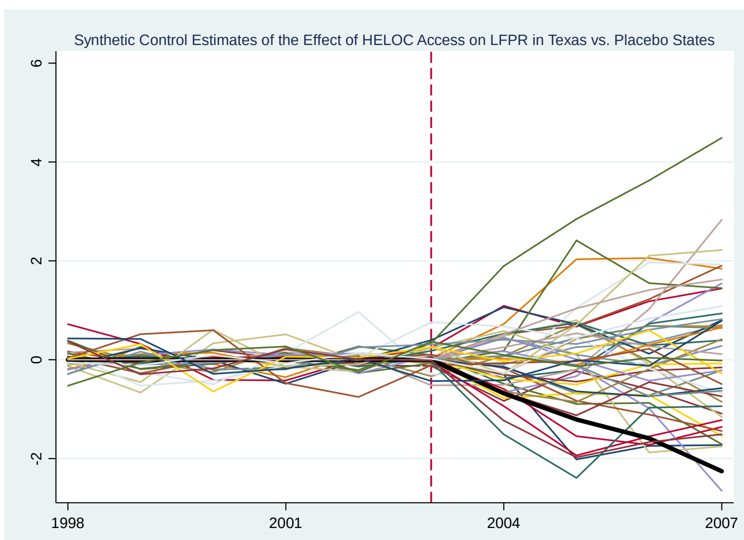
The figure plots the difference between LFPR paths of each state and its synthetic control for the specification described in notes to Figure 2A, with the difference between Texas and synthetic Texas presented in solid bold. The figure shows that just a handful of placebo states have post-treatment LFPR relative to their synthetic counterparts as negative as Texas. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 3A



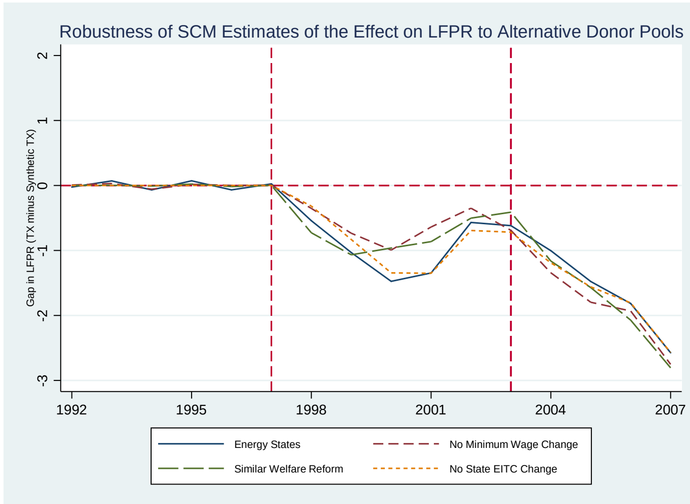
The figure shows the pre-HELOC (1998-2003) and post-HELOC (2004-2007) LFPR path for the treatment group (Texas) and the control group (synthetic-Texas) using the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for Texas. 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 (2016). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 3B



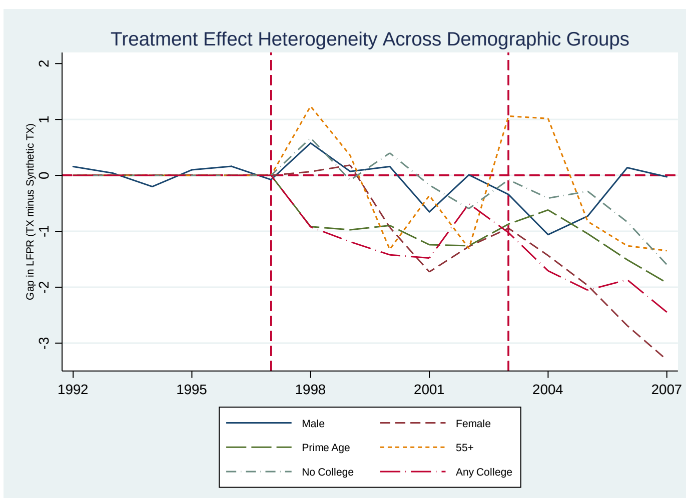
The figure plots the difference between LFPR paths of each state and its synthetic control for the specification described in notes to Figure 3A, with the difference between Texas and synthetic Texas presented in solid bold. The figure shows that just a handful of placebo states have post-treatment LFPR 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, 2016). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 4A



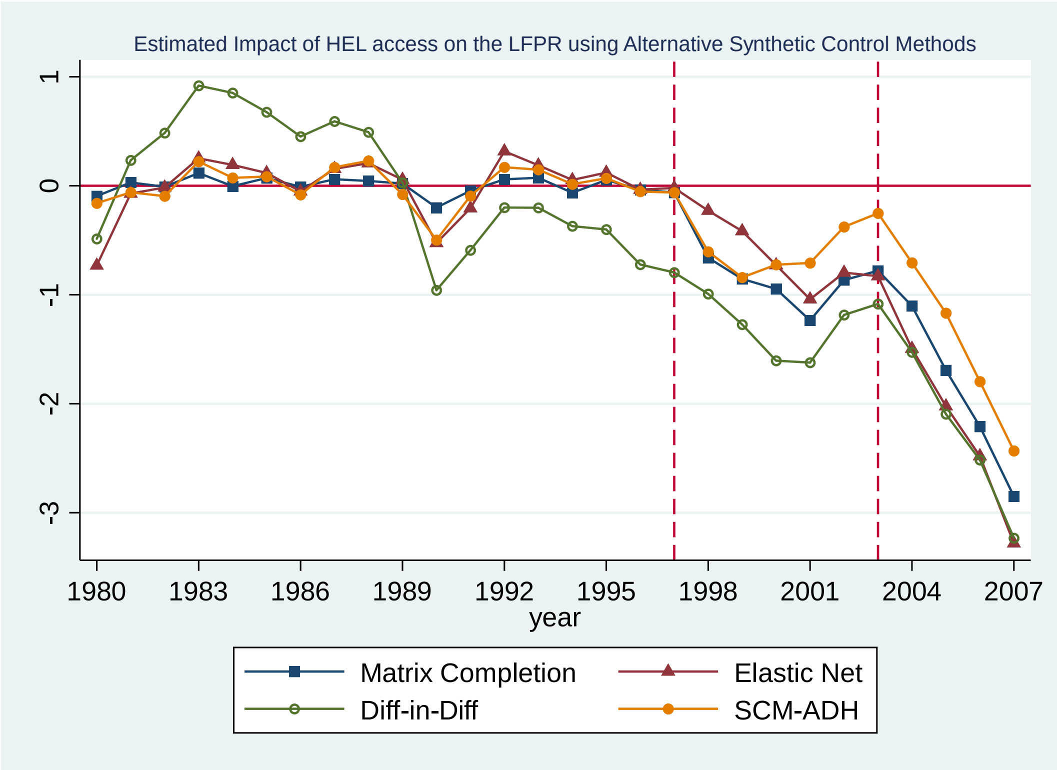
Notes: For alternative donor pools, the figure plots the difference between LFPR paths of Texas and synthetic Texas for the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for Texas. Vertical dashed lines denote 1997 and 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 (2016). Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 4B



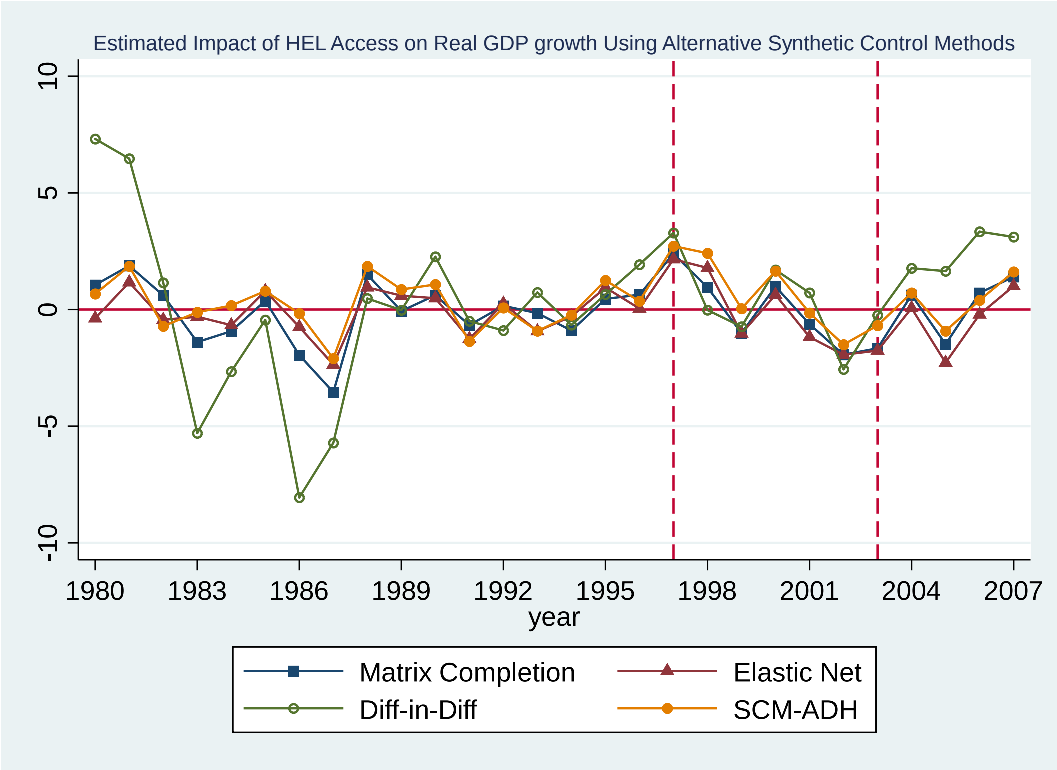
Notes: Using grouped basic monthly CPS data by state, year and demographic groups from 1992-2007, the figure plots the difference between LFPR paths of Texas and synthetic Texas for the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for 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 for all demographic groups, yet the post-treatment paths diverge significantly for most. All analysis using synthetic control estimation is carried out using "Synth" package and "Synth Runner" packages Abadie at al. (2014), Galiani and Quistorff (2016). Data Sources: Basic CPS-IPUMS; Authors' calculations.

Figure 5



Notes: The figure plots the pre-HEL (1980-1997) and post-HEL (1998-2007) difference between LFPR paths of the treatment group (Texas) and the weighted average of control states (synthetic-Texas) using the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for Texas. The estimates plotted are for alternative synthetic control methods reported in Table 4. 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, except the DID, yet the post-treatment paths diverge significantly. Estimation carried out using software code for SCM with Elastic Net penalty available from Doudchenko and Imbens (2016) and DID/SCM-ADH/MC-NNM code from https://github.com/susanathey/MCPanel. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Figure 6



Notes: The figure plots the pre-HEL (1980-1997) and post-HEL (1998-2007) difference between real GDP growth paths of the treatment group (Texas) and the weighted average of control states (synthetic-Texas) using the constrained regression model that uses all pre-treatment lags of the outcome variable (real GDP growth) to construct the synthetic control for Texas. The estimates plotted are for alternative synthetic control methods reported in Table 5. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. Estimation carried out using software code for SCM with Elastic Net penalty available from Doudchenko and Imbens (2016) and DID/SCM-ADH/MC-NNM code from https://github.com/susanathey/MCPanel. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Table 1: Difference in Differences Estimates of Home Equity Access on LFPR

	(1)	(2)	(3)	(4)
		Panel A: All States Sample		
Texas X 1998-2003	-1.080	-0.764	-0.410	-0.502
	(0.144)	(0.117)	(0.498)	(0.436)
	[-1.723, 0.140]	[-1.281, 0.136]	[-1.174, 0.285]	[-1.208, 0.095]
Texas X Post 2003	-2.069	-1.198	-1.566	-1.267
	(0.219)	(0.225)	(0.359)	(0.683)
	[-3.811, -0.808]	[-2.819, -0.079]	[-2.696, -0.541]	[-1.731, -0.724]
		Panel B: Energy States Sample		
Texas X 1998-2003	-1.152	-0.688	-1.015	-0.834
	(0.152)	(0.130)	(0.271)	(0.356)
	[-2.012, -0.595]	[-1.589, -0.077]	[-1.689, -0.619]	[-1.360, -0.386]
Texas X Post 2003	-2.357	-1.722	-2.332	-1.566
	(0.290)	(0.292)	(0.437)	(0.839)
	[-5.826, -1.759]	[-3.792, -1.091]	[-3.260, -1.649]	[-1.920, -1.220]
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Other Covariates	No	Yes	Yes	Yes
Division X Year Effects	No	No	Yes	Yes
State X Linear Trend	No	No	No	Yes
Observations (Panel A)	800	797	797	797
AdjR-Sq (Panel A)	0.943	0.963	0.969	0.978

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 high school, and share with a college degree. Data Sources: BLS/LAUS; Haver Analytics; Basic CPS-IPUMS; Authors' calculations.

Table 2: Heterogeneity in DID Estimates of Home Equity Access on LFPR

	(1) Male	(2) Female	(3) Prime-Age	(4) Age-55+	(5) No-College	(6) Any-College
Panel A: DID Estimates without State-Specific Linear Time Trends						
Texas X 1998-2003	-0.376	-0.639	-1.436	1.477	0.269	-1.886
	(0.536)	(0.802)	(0.381)	(0.890)	(0.631)	(0.745)
	[-1.226, 1.143]	[-1.671, 0.725]	[-2.312, -0.567]	[-0.659, 3.363]	[-0.847, 1.679]	[-2.845, -0.599]
Texas X Post 2003	-1.165	-2.245	-1.819	0.004	-1.297	-2.671
	(0.230)	(0.716)	(0.635)	(0.303)	(0.796)	(0.922)
	[-3.313, 0.112]	[-4.352, -0.893]	[-3.485, -0.517]	[-2.755, 3.047]	[-3.496, 0.471]	[-4.714, -1.408]
Panel B: DID Estimates with State-Specific Linear Time Trends						
Texas X 1998-2003	-0.094	0.069	-0.910	0.428	1.310	-2.253
	(0.724)	(0.866)	(0.244)	(1.934)	(0.561)	(0.868)
	[-0.965, 1.369]	[-1.007, 1.350]	[-1.825, -0.077]	[-1.718, 2.652]	[0.181, 2.491]	[-3.254, -0.895]
Texas X Post 2003	-0.646	-0.969	-0.876	-1.900	0.586	-3.341
	(0.609)	(0.693)	(0.816)	(2.210)	(0.428)	(1.034)
	[-1.699, 0.071]	[-1.667, -0.306]	[-1.656, -0.135]	[-3.238, -0.496]	[-0.277, 1.237]	[-4.462, -2.666]
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Division X Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63372	65417	48508	41464	67219	61570
AdjR-Sq	0.890	0.891	0.675	0.753	0.876	0.839

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, indicators for demographic groups as controls, and division X year effects. Data Sources: Basic Monthly CPS; Authors' calculations.

Table 3: Synthetic Control Estimates with Standardized P-Values

	Model with All Pre-Treatment Lags	Model with Covariates and Some Lags	Model with All Pre-Treatment Lags: Energy States	Model with All Pre-Treatment Lags: Homeowners	Model with All Pre-Treatment Lags: Renters
1998	-0.267	-0.527	-0.542	-1.354	0.337
	[0.000]	[0.163]	[0.091]	[0.082]	[0.061]
1999	-0.545	-0.988	-1.032	-2.133	0.342
	[0.000]	[0.061]	[0.091]	[0.061]	[0.061]
2000	-0.605	-1.566	-1.475	-1.383	-0.617
	[0.000]	[0.041]	[0.091]	[0.082]	[0.082]
2001	-0.777	-1.804	-1.346	-2.061	2.598
	[0.000]	[0.082]	[0.091]	[0.061]	[0.020]
2002	-0.565	-1.146	-0.569	-0.180	1.906
	[0.000]	[0.122]	[0.091]	[0.143]	[0.020]
2003	-0.496	-1.181	-0.618	-1.267	0.879
	[0.000]	[0.163]	[0.091]	[0.082]	[0.020]
2004	-0.869	-1.444	-1.003	-1.103	-0.935
	[0.000]	[0.061]	[0.091]	[0.102]	[0.061]
2005	-1.459	-1.923	-1.477	-1.508	-3.320
	[0.000]	[0.041]	[0.091]	[0.102]	[0.000]
2006	-1.985	-2.240	-1.816	-1.655	-2.865
	[0.000]	[0.020]	[0.091]	[0.082]	[0.020]
2007	-2.554	-2.969	-2.574	-1.650	-0.513
	[0.000]	[0.020]	[0.091]	[0.082]	[0.082]
Treatment Effect	-1.012	-1.579	-1.245	-1.429	-0.219
Std. P-value	0	0.0408	0.0909	0.102	0.0204
Pre-Mean Effect	9.47e-13	-0.0675	0.00121	-3.25e-11	1.06e-11
Pre-Std. P-value	1	0.857	0.909	0.898	0.980
Pre-RMSPE: TX	1.96e-10	0.151	0.0586	1.03e-09	1.18e-10
Pre-RMSPE: Ctrls	0.309	0.498	1.152	0.590	0.979

Standardized P-values reported in square brackets. Pre-treatment period: 1992-1997; Post-treatment period: 1998-2007; Treated group: Texas; Control Group: 49 remaining states. The table shows synthetic control estimates of the treatment effect, i.e. the post-treatment (post-1997) difference between LFPR of the treatment group (Texas) and the synthetic-Texas for the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for Texas. All analysis using synthetic control estimation is carried out using the "Synth" and "Synth Runner" packages (Abadie at al. 2014, Galiani and Quistorff, 2016). Sources: BLS-LAUS; Authors' calculations.

Table 4: Estimated Treatment Effects of Home Equity Access on LFPR from Alternative SCM Methods with Standardized P-Values

	(1) Diff-in-Diff	(2) SCM	(3) Elastic Net	(4) Matrix Completion
1998	-0.992	-0.605	-0.228	-0.661
	[0.265]	[0.102]	[0.673]	[0.224]
1999	-1.273	-0.841	-0.416	-0.854
	[0.122]	[0.0408]	[0.612]	[0.245]
2000	-1.605	-0.724	-0.728	-0.947
	[0.0408]	[0.184]	[0.286]	[0.122]
2001	-1.622	-0.708	-1.041	-1.235
	[0.102]	[0.163]	[0.224]	[0.122]
2002	-1.185	-0.377	-0.797	-0.865
	[0.163]	[0.469]	[0.327]	[0.327]
2003	-1.084	-0.252	-0.829	-0.779
	[0.184]	[0.571]	[0.306]	[0.327]
2004	-1.527	-0.706	-1.495	-1.103
	[0.184]	[0.245]	[0.265]	[0.286]
2005	-2.094	-1.168	-2.023	-1.693
	[0.102]	[0.102]	[0.122]	[0.0408]
2006	-2.516	-1.796	-2.481	-2.209
	[0.0408]	[0.0816]	[0.0408]	[0.0204]
2007	-3.233	-2.432	-3.281	-2.849
	[0.0408]	[0.0408]	[0.0204]	[0.0408]
Treatment Effect	-1.713	-0.961	-1.332	-1.320
Std. P-value	0.0816	0.0816	0.0816	0.0816
Pre-Mean Effect	8.68e-15	-0.000237	2.37e-15	0.0000518
Pre-Std. P-value	0.816	0.898	0.776	0.918
Pre-RMSPE: TX	0.585	0.169	0.258	0.0738
Pre-RMSPE: Ctrls	1.000	0.620	0.321	0.102

Notes: Standardized P-values reported in square brackets. Pre-treatment period: 1980-1997; Post-treatment period: 1998-2007; Treated group: Texas; Control Group: 49 remaining states. Using alternative synthetic control methods, the table shows estimates of the treatment effect, i.e. the post-treatment (post-1997) difference between LFPR of the treatment group (Texas) and the synthetic-Texas for the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for Texas. Estimation carried out using software code for SCM with Elastic Net penalty available from Doudchenko and Imbens (2016) and DID/SCM-ADH/MC-NNM code from https://github.com/susanathey/MCPanel.

Table 5: Estimated Treatment Effects of Home Equity Access on Real GDP Growth with Standardized P-Values

	(1) Diff-in-Diff	(2) SCM	(3) Elastic Net	(4) Matrix Completion
1998	-0.0213	2.414	1.783	0.950
	[1]	[0.184]	[0.429]	[0.633]
1999	-0.734	0.0385	-1.024	-1.010
	[0.714]	[0.959]	[0.490]	[0.551]
2000	1.699	1.651	0.630	0.986
	[0.571]	[0.388]	[0.755]	[0.694]
2001	0.717	-0.148	-1.179	-0.623
	[0.898]	[0.918]	[0.429]	[0.857]
2002	-2.562	-1.502	-1.938	-1.935
	[0.306]	[0.408]	[0.245]	[0.224]
2003	-0.243	-0.685	-1.758	-1.662
	[0.980]	[0.571]	[0.224]	[0.388]
2004	1.771	0.713	0.0606	0.625
	[0.612]	[0.735]	[0.959]	[0.735]
2005	1.646	-0.922	-2.279	-1.485
	[0.735]	[0.694]	[0.306]	[0.490]
2006	3.339	0.399	-0.211	0.711
	[0.429]	[0.694]	[0.918]	[0.653]
2007	3.111	1.616	1.009	1.403
	[0.510]	[0.490]	[0.612]	[0.592]
Treatment Effect	0.872	0.357	-0.491	-0.204
Std. P-value	0.959	0.837	0.837	0.959
Pre-Mean Effect	1.17e-16	0.337	3.70e-17	0.00304
Pre-Std. P-value	0.143	0.429	0.429	0.367
Pre-RMSPE: TX	3.741	1.218	1.017	1.386
Pre-RMSPE: Ctrls	2.980	1.541	1.119	1.328

Notes: Standardized P-values reported in square brackets. Pre-treatment period: 1980-1997; Post-treatment period: 1998-2007; Treated group: Texas; Control Group: 49 remaining states. Using alternative synthetic control methods, the table shows estimates of the treatment effect, i.e. the post-treatment (post-1997) difference between LFPR of the treatment group (Texas) and the synthetic-Texas for the constrained regression model that uses all pre-treatment lags of the outcome variable (real GDP growth) to construct the synthetic control for Texas. Estimation carried out using software code for SCM with Elastic Net penalty available from Doudchenko and Imbens (2016) and DID/SCM-ADH/MC-NNM code from https://github.com/susanathey/MCPanel.

Appendix A

Figure A1

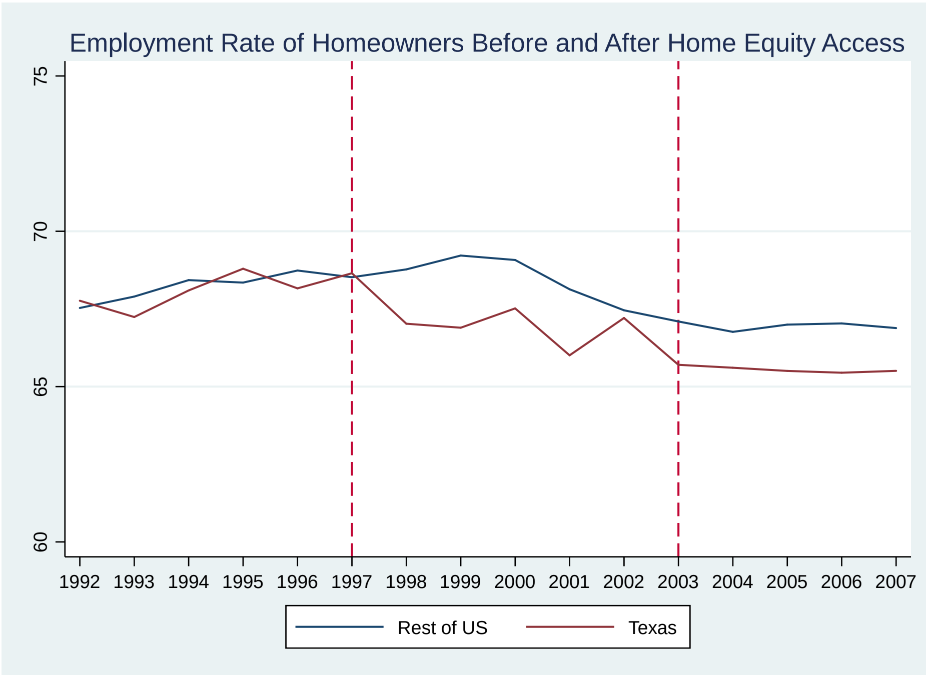
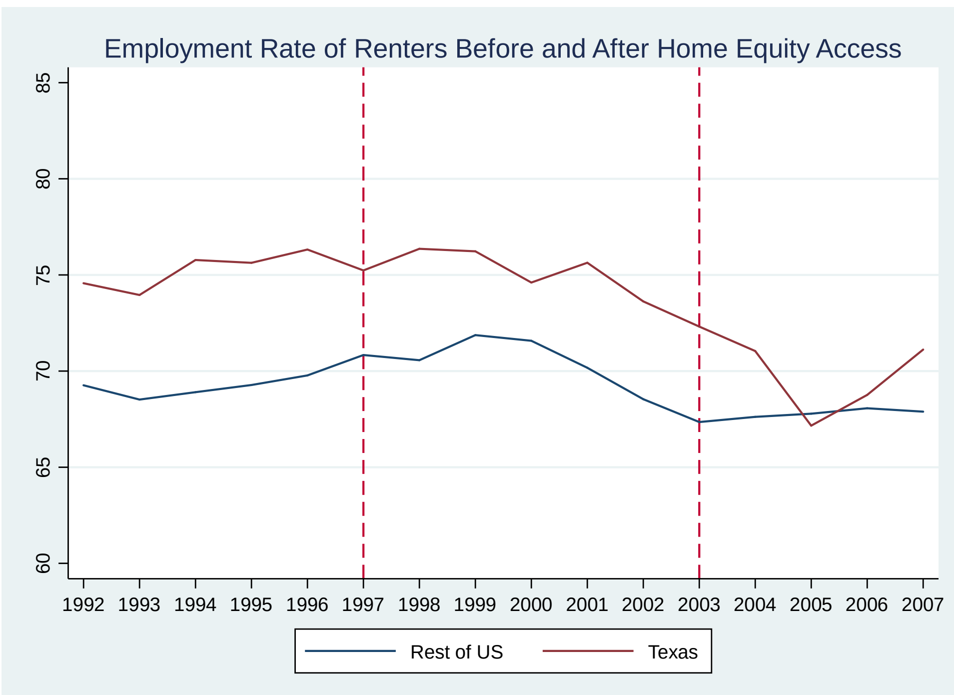
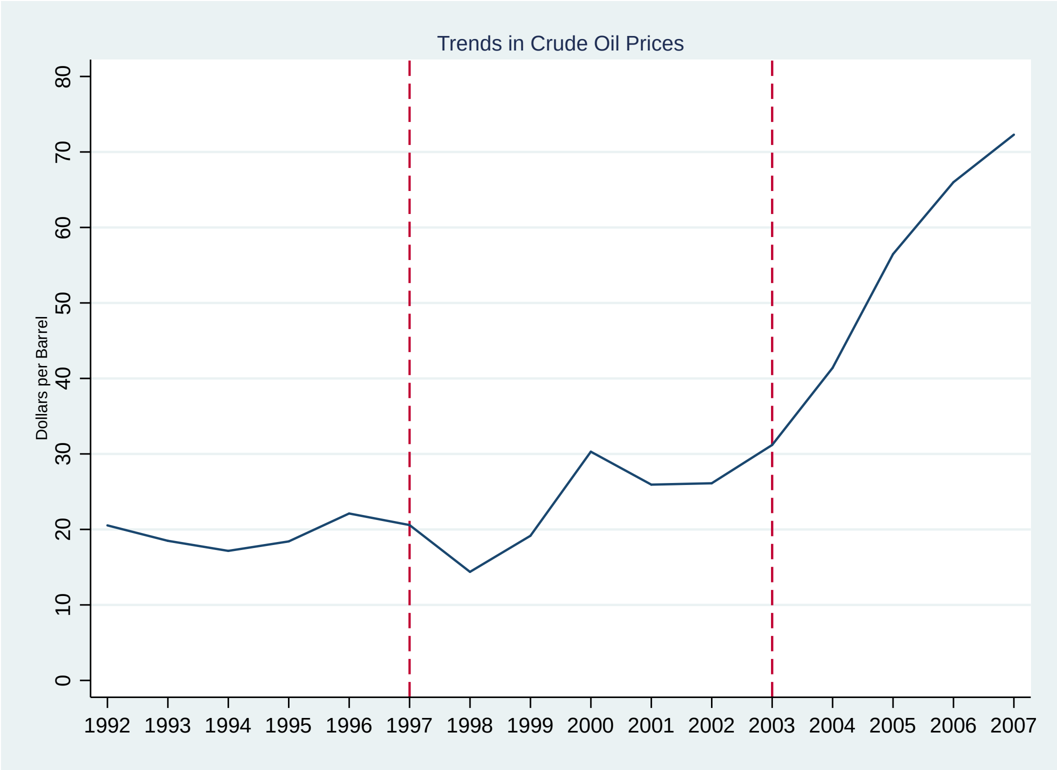


Figure A2



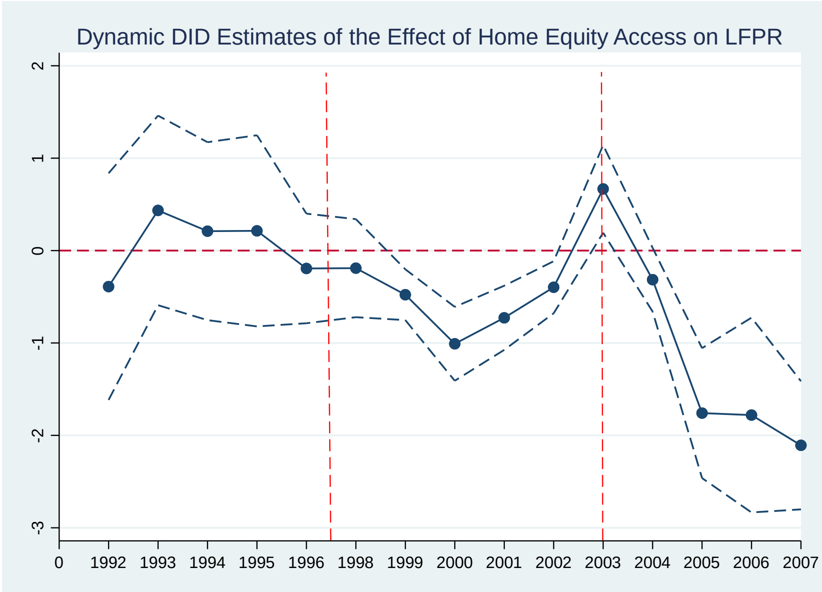
Notes: Using March CPS-IPUMS data, the figure plots the weighted-average labor force participation rate for Texas and the remaining states for homeowners (top panel) and renters (bottom panel). Averages weighted by household weight variable in March CPS (hwtsupp). Sources: IPUMS-CPS; Authors' calculations.

Figure A3



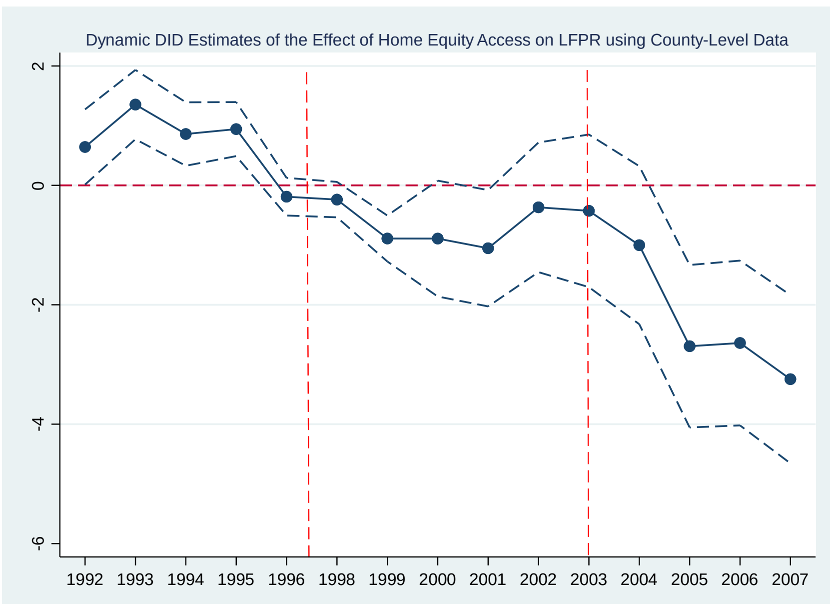
Sources: Department of Energy; Haver Analytics.

Figure A4



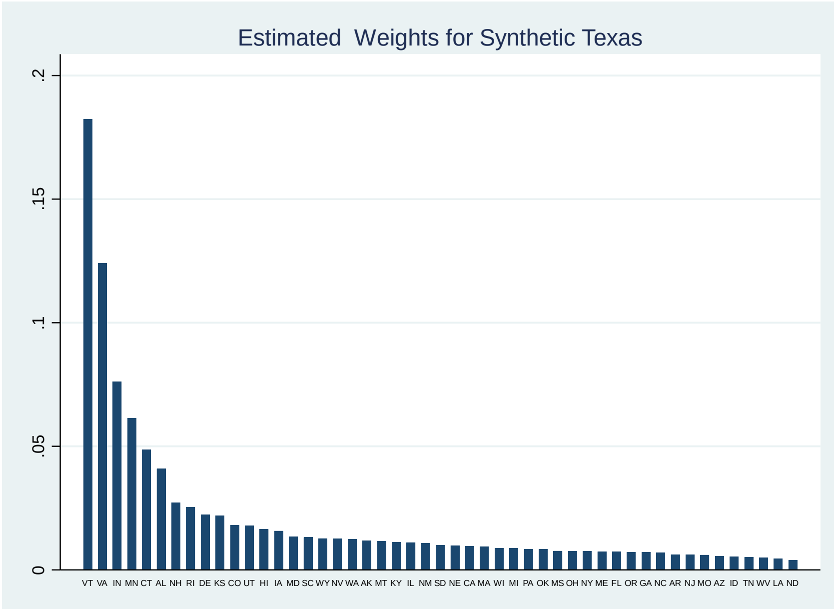
Notes: Using state-level data, the figure plots coefficients on the interactions between the treatment dummy (an indicator for Texas) and dummies for each year from 1992 to 2007 from a regression of the LFPR on those interactions, state fixed effects, year fixed effects, key economic and demographic covariates, and census-division specific year effects (the specification reported in columns 3 of Appendix Table A2). 1997 is the omitted base year, with its interaction with the treatment dummy normalized to zero, so that estimates should be interpreted as the difference between Texas and rest of U.S. relative to the difference in year 1997—the year just before the law change. Vertical dashed lines denote 1997 and 2003, the years of introduction of HEL and HELOC, respectively. Sources: BLS/LAUS; Authors' calculations.

Figure A5



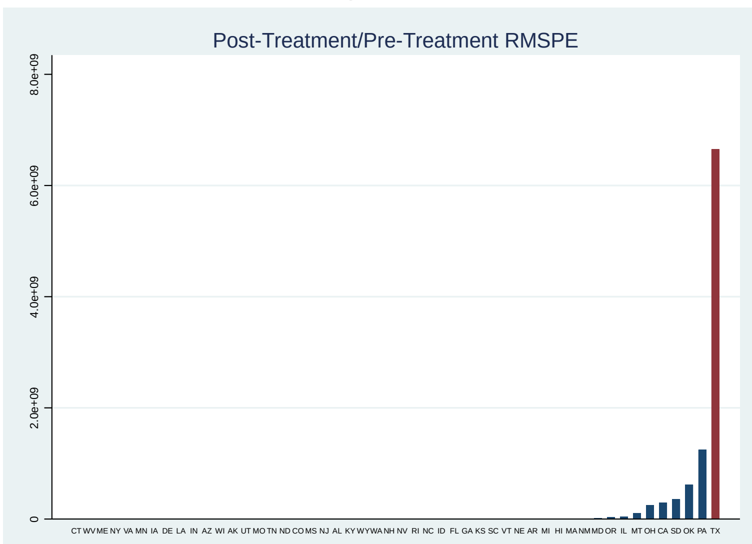
Note: Using county-level data, the figure plots coefficients from the specification reported in columns 3 of Appendix Table A3. Sources: BLS/LAUS; Authors' calculations.

Figure A6



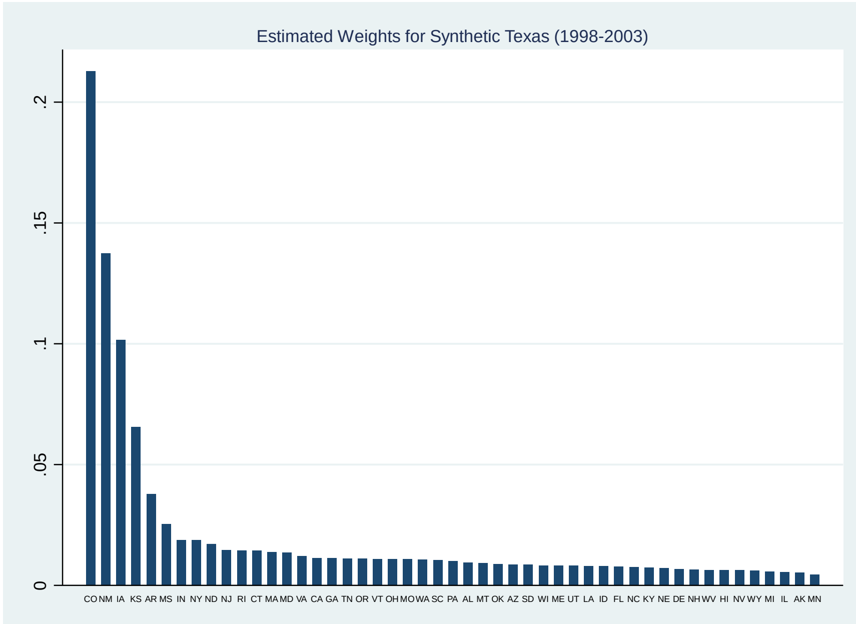
The figure shows the estimated weights for different states in constructing the counterfactual for Texas (synthetic Texas) for the synthetic control estimates plotted in Figure 2A/2B and reported in column (1) of Table 3. Weights are chosen to minimize the mean squared prediction error (MSPE) between pre-treatment characteristics of the treatment group (Texas) and its synthetic control (synthetic Texas), using all pre-treatment lags of the outcome variable (LFPR). See notes to Figure 2A/2B and Table 3 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, 2016). Sources: BLS/LAUS; Authors' calculations.

Figure A7



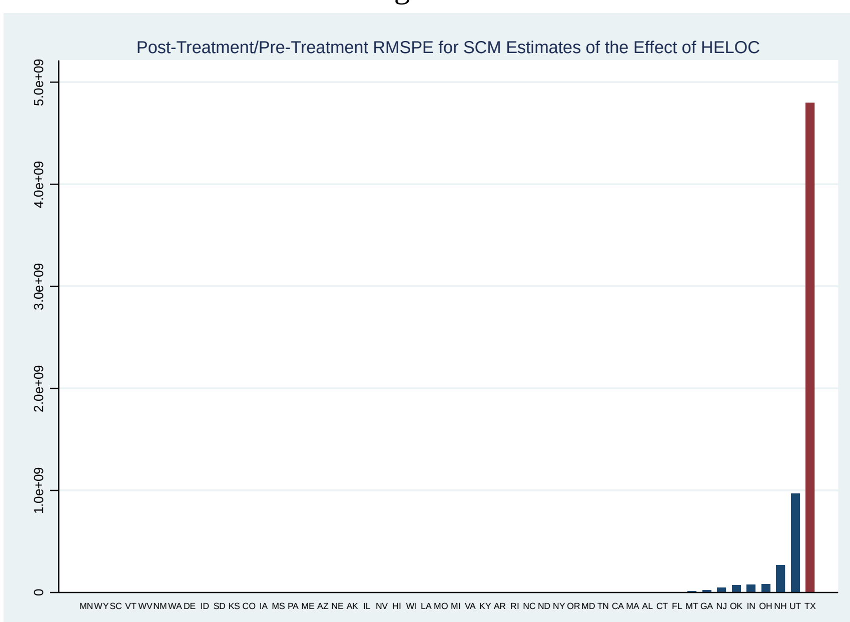
The figure plots the ratio of post-treatment (post-1997) RMSPE to the pre-treatment (pre-1997) RMSPE of the treated state (Texas) and other control states for the synthetic control estimates plotted in Figure 2A/2B and reported in column (1) of Table 3. RMSPE for each state is simply the square root 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 A6. The figure shows that the post-1997 difference in LFPR of Texas and its counterfactual (synthetic Texas) relative to the pre-1997 difference is the largest of all states. Sources: BLS/LAUS; Authors' calculations.

Figure A8



The figure shows the estimated weights for different states in constructing the counterfactual for Texas (synthetic Texas) for the synthetic control estimates plotted in Figure 3A/3B. The figure is analogous to Figure A6, except that it plots estimated weights for SCM-ADH estimated effects of HELOC in the post-2003 period. See notes to Figure A6 for more details.

Figure A9



The figure plots the ratio of post-HELOC (2004-2007) RMSPE to the pre-HELOC (1998-2003) RMSPE of the treatment state (Texas) vs. other states for the synthetic control estimates plotted in Figure 3A/3B. The figure is analogous to Figure A7, except that it uses SCM-ADH estimates of HELOC in the post-2003 period. See notes to Figure A7 for more details.

Figure A10

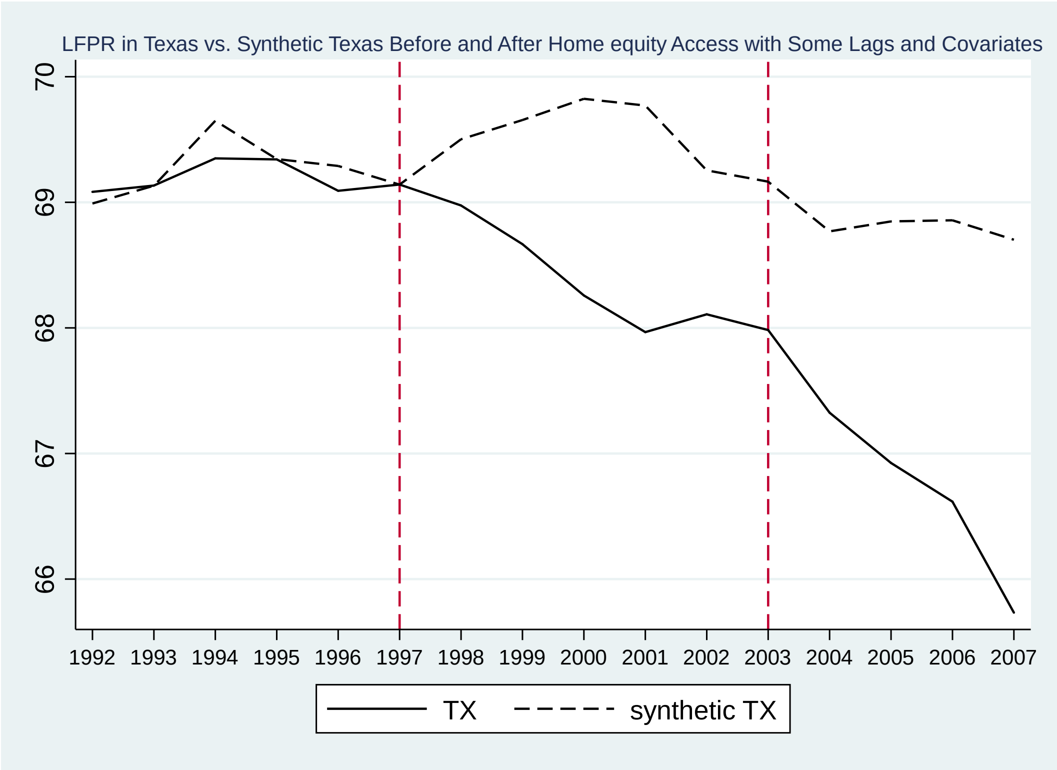


Figure A11

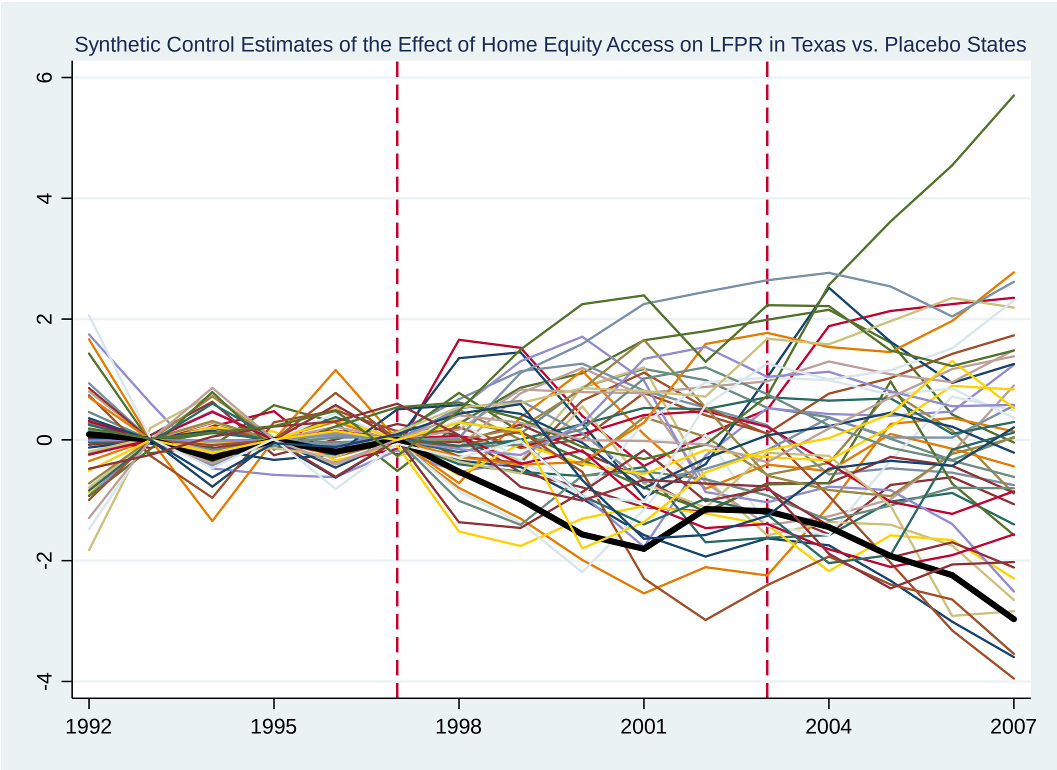
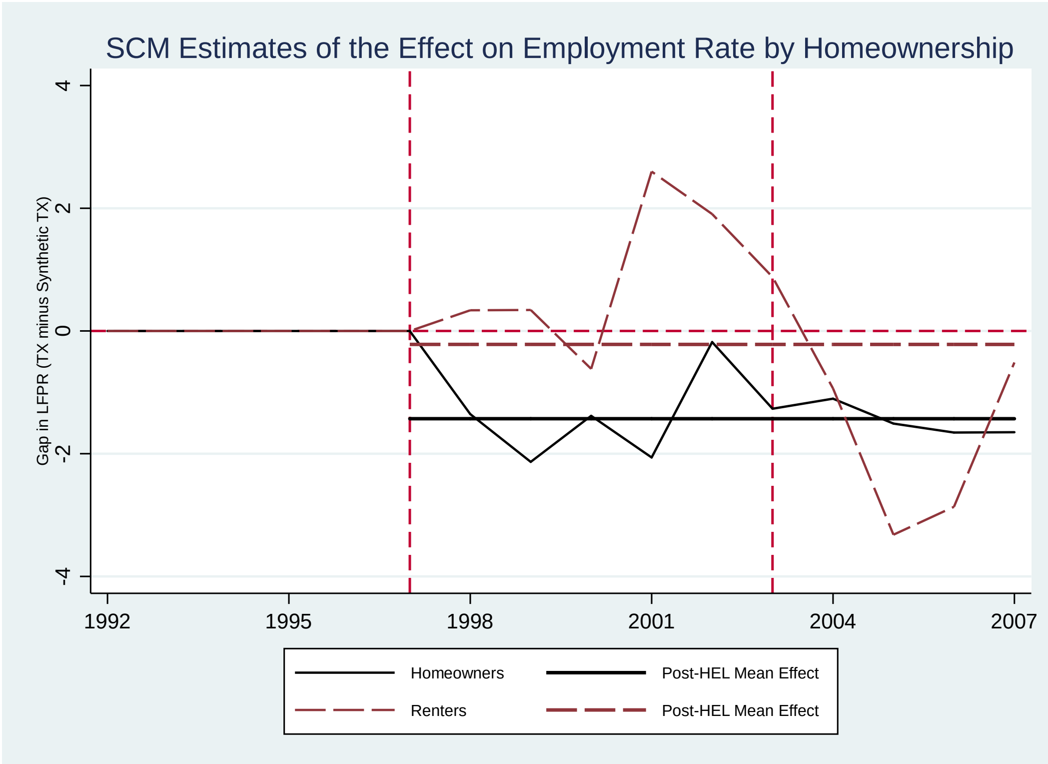


Figure A12



Using grouped March CPS-IPUMS data for homeowners and renters, the figure plots the difference between LFPR paths of Texas and synthetic Texas for the constrained regression model that uses all pre-treatment lags of the outcome variable (LFPR) to construct the synthetic control for Texas. The figure shows that the pre-treatment path of LFPR of "synthetic Texas" is almost identical to that for Texas for both homeowners and renters, yet the post-treatment paths diverge significantly for homeowners and there is no clear trend for renters. The post-HEL mean effect is large for homeowners and relatively very small for renters. All analysis using synthetic control estimation is carried out using "Synth" package and "Synth Runner" packages Abadie at al. (2014), Galiani and Quistorff (2016). Data Sources: March CPS-IPUMS; Authors' calculations.

Figure A13

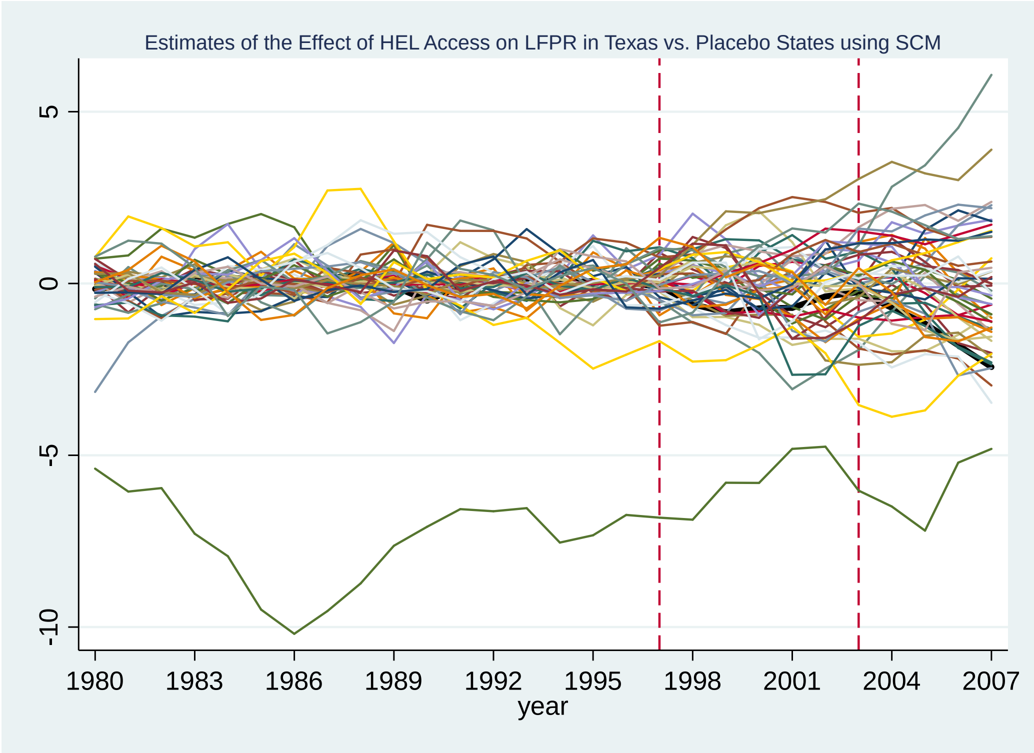


Figure A14

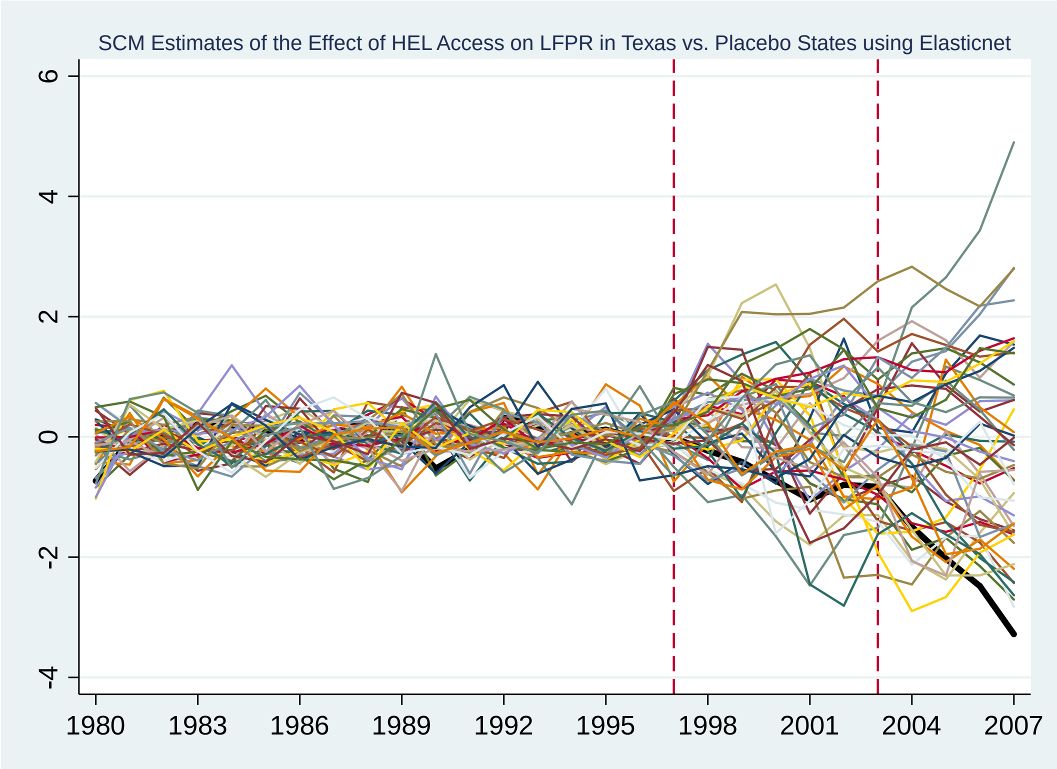


Figure A15

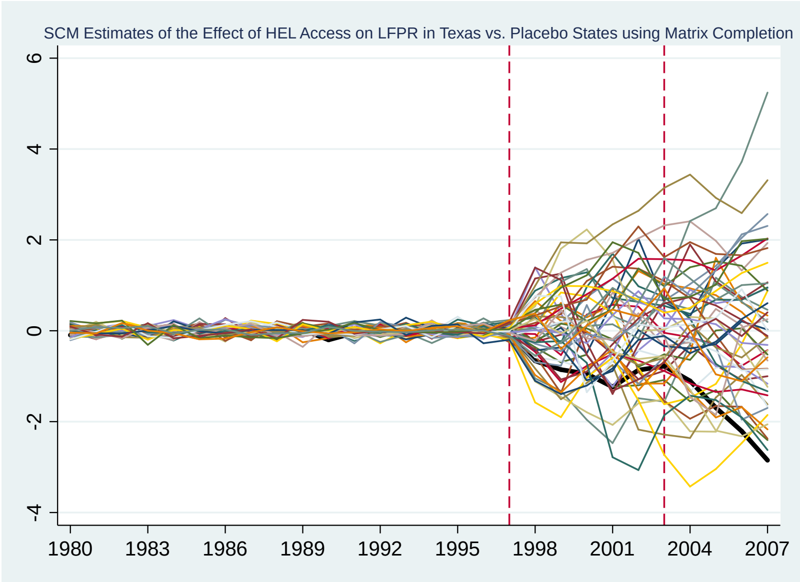
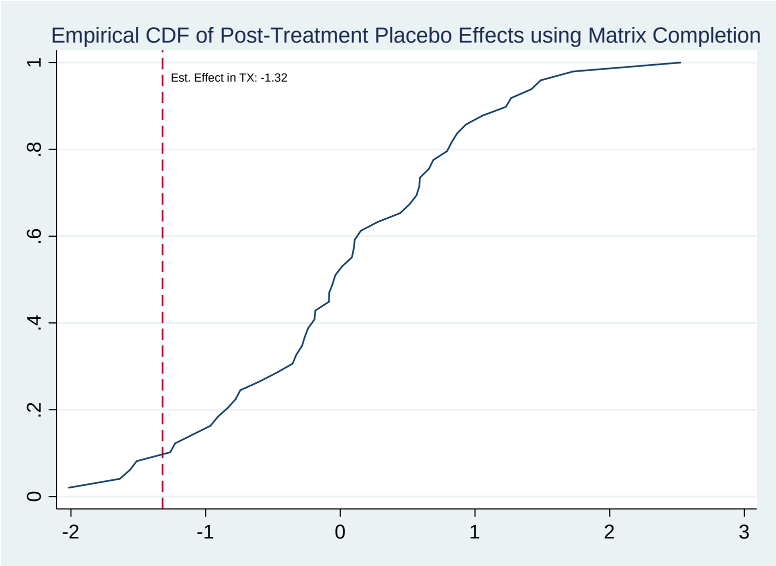


Figure A16



The figure plots the empirical CDF of MC-NNM estimates of 10-year (1998-2007) average effects across states for MC-NNM estimates reported in Table 4 and Figure 5.
Sources: March CPS-IPUMS; Authors' calculations.

Figure A17

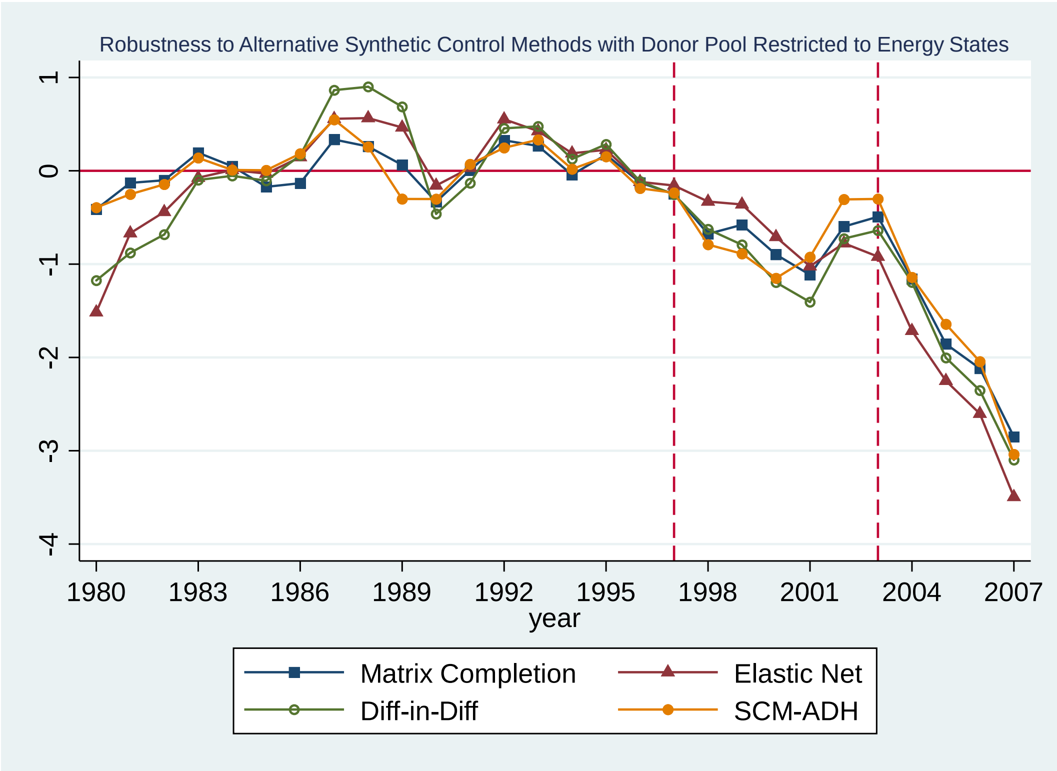


Figure A18

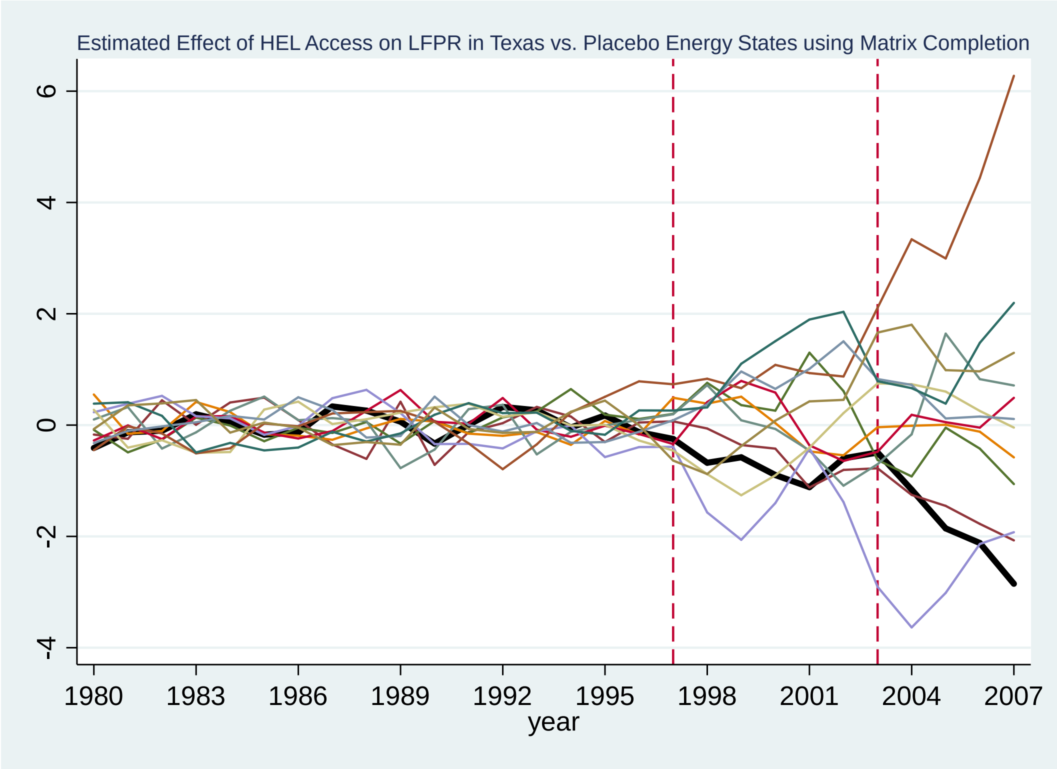


Table A1: Summary Statistics

	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 A2: Time-varying Difference in Differences Estimates of Home Equity Access on LFPR using State-Level Data

	(1)	(2)	(3)	(4)
1992	0.63127	0.64802	-0.38987	
	(0.229)	(0.159)	(0.732)	
1993	0.80334	0.69861	0.43466	
	(0.175)	(0.142)	(0.612)	
1994	0.76639	0.65835	0.21006	
	(0.181)	(0.152)	(0.574)	
1995	0.71015	0.72102	0.21365	
	(0.186)	(0.169)	(0.617)	
1996	0.29280	0.17418	-0.19346	
	(0.111)	(0.121)	(0.354)	
1998	-0.16751	-0.36178	-0.19037	-0.25411
	(0.079)	(0.107)	(0.317)	(0.494)
1999	-0.48739	-0.46684	-0.47751	-0.29827
	(0.118)	(0.098)	(0.164)	(0.418)
2000	-0.91417	-1.00487	-1.00895	-0.47769
	(0.180)	(0.163)	(0.238)	(0.492)
2001	-0.94969	-0.52108	-0.72776	-0.13961
	(0.213)	(0.156)	(0.207)	(0.471)
2002	-0.51978	-0.02806	-0.39678	0.27748
	(0.214)	(0.175)	(0.168)	(0.498)
2003	-0.27963	0.47036	0.66696	1.58027
	(0.177)	(0.181)	(0.283)	(0.705)
2004	-0.70975	-0.09650	-0.31473	0.73083
	(0.195)	(0.211)	(0.206)	(0.721)
2005	-1.23453	-0.51133	-1.75868	-0.47843
	(0.218)	(0.222)	(0.419)	(1.272)
2006	-1.68231	-0.92287	-1.77997	-0.32344
	(0.227)	(0.228)	(0.629)	(1.425)
2007	-2.48743	-1.42771	-2.10719	-0.30547
	(0.269)	(0.283)	(0.414)	(1.321)
State and Year Fixed Effects	Yes	Yes	Yes	Yes
Other Covariates	No	Yes	Yes	Yes
Division X Year Effects	No	No	Yes	Yes
State X Linear Trend	No	No	No	Yes
Observations	800	797	797	797
AdjR-Sq	0.9434	0.9635	0.9693	0.9788

Notes: Robust standard errors clustered by state are reported in parenthesis. Estimation weighted by state population. Using state-level data from 1992-2007, the table reports coefficients on the interactions between the treatment (Texas) dummy and year dummies for 1992 to 2007 from a regression of the LFPR on Texas X Year interactions, state and year effects (in column 1), and other controls, as indicated, in column 2-4. 1997 is the omitted base year, with its interaction with the treatment dummy normalized to zero, so that estimates should be interpreted as the difference between Texas and rest of U.S. relative to the difference in year 1997—the year just before the law change. See notes to Table 1 for details on additional covariates included in columns 2-4. Pre-treatment interactions are excluded in column 4 to identify state-specific linear time trends. Sources: BLS-LAUS; Authors' calculations.

Table A3: Time-varying Difference in Differences Estimates of Home Equity Access on LFPR using County-Level Data

	(1)	(2)	(3)	(4)
1992	1.49834	1.40140	0.64306	
	(0.298)	(0.310)	(0.383)	
1993	1.41529	1.28691	1.35349	
	(0.256)	(0.267)	(0.353)	
1994	1.50619	1.34538	0.86031	
	(0.203)	(0.211)	(0.323)	
1995	1.36632	1.23068	0.94277	
	(0.160)	(0.166)	(0.275)	
1996	0.55892	0.47353	-0.18982	
	(0.122)	(0.119)	(0.192)	
1998	0.35756	0.32645	-0.23817	-0.03937
	(0.133)	(0.144)	(0.180)	(0.256)
1999	-0.20908	-0.27384	-0.89012	-0.45954
	(0.154)	(0.166)	(0.234)	(0.305)
2000	-1.54467	-1.69093	-0.89118	-0.23172
	(0.503)	(0.511)	(0.590)	(0.621)
2001	-1.56809	-1.73809	-1.05306	-0.16359
	(0.490)	(0.493)	(0.592)	(0.678)
2002	-0.83465	-0.99346	-0.36736	0.75115
	(0.529)	(0.532)	(0.660)	(0.797)
2003	-0.58730	-0.77286	-0.42593	0.92125
	(0.661)	(0.655)	(0.777)	(0.953)
2004	-0.92318	-1.04746	-1.00133	0.57404
	(0.694)	(0.690)	(0.804)	(1.024)
2005	-1.63405	-1.60630	-2.69430	-0.89124
	(0.693)	(0.694)	(0.827)	(1.093)
2006	-2.48710	-2.33464	-2.64006	-0.60960
	(0.679)	(0.674)	(0.839)	(1.165)
2007	-3.13346	-2.96846	-3.24586	-0.98695
	(0.671)	(0.661)	(0.856)	(1.271)
County and Year Fixed Effects	Yes	Yes	Yes	Yes
House Price	No	Yes	Yes	Yes
Division X Year Effects	No	No	Yes	Yes
State X Linear Trend	No	No	No	Yes
Observations	46672	33717	33717	33717
AdjR-Sq	0.8873	0.8931	0.9017	0.9069

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 coefficients on the interactions between the treatment dummy (an indicator for Texas) and dummies for each year from 1992 to 2007 from a regression of the LFPR on Texas X Year interactions, state effects, year effects (in column 1), and other controls, as indicated, in column 2-4. 1997 is the omitted base year, with its interaction with the treatment dummy normalized to zero, so that estimates should be interpreted as the difference between Texas and rest of U.S. relative to the difference in year 1997—the year just before the law change. Pre-treatment interactions are excluded in column 4 to identify state-specific linear time trends. Sources: BLS-LAUS; Authors' calculations.

Appendix B

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 conditions imply, as we write in the main text, the following optimality conditions:

$$u_{c_1} = \frac{u_{l_1}}{w} = \frac{(1+r)\delta u_{l_2}}{w} + \mu_4,$$

$$\delta u_{c_2} = \frac{\delta u_{l_2}}{w} = (1+r)\delta^2 u_{c_3} + \mu_5.$$

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:

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

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 in the main text.