A Crisis of Missed Opportunities? Foreclosure Costs and Mortgage Modification During the Great Recession*

Stuart Gabriel
University of California, Los Angeles

Matteo Iacoviello
Federal Reserve Board

Chandler Lutz
Copenhagen Business School

December 15, 2018

Abstract

We investigate the causal housing impacts of the 2000s crisis-period California Foreclosure Prevention Laws (CFPLs), policies that encouraged mortgage modifications by substantially increasing the lender pecuniary and time costs of foreclosure. The CFPLs prevented 248,000 California foreclosures (a reduction of 20.9%), increased California aggregate house prices by 6.2%, and created \$350 billion of housing wealth. Findings also indicate that the CFPLs increased maintenance and repair spending for homes that entered foreclosure, mitigating foreclosure externalities, while also boosting mortgage modifications. The CFPLs had minimal adverse side effects in terms of the availability of mortgage credit for new borrowers. Altogether, the CFPLs were a highly effective foreclosure intervention that required no pecuniary subsidy from taxpayers.

JEL Classification: E52, E58, R20, R30;

Keywords: Foreclosure Crisis, Mortgage Modification, Great Recession

*Gabriel: stuart.gabriel@anderson.ucla.edu. Iacoviello: matteo.iacoviello@frb.gov. Lutz: cl.eco@cbs.dk.
Gabriel acknowledges funding from the UCLA Gilbert Program in Real Estate, Finance, and Urban Economics. Lutz acknowledges funding from the UCLA Ziman Center for Real Estate's Howard and Irene Levine Program in Housing and Social Responsibility. Data and code available at https://github.com/ChandlerLutz/CFPLCode.

1 Introduction and Background

At the height of the housing boom in 2005, California accounted for one-quarter of US housing wealth.[1] But as 2006 boom turned into 2008 bust, house prices in the state fell 30 percent and over 800,000 homes entered foreclosure.[2] To aid distressed borrowers, limit foreclosures, and combat the crisis, the State of California pursued an alternative policy strategy that increased foreclosure pecuniary costs and imposed foreclosure moratoria to incent widespread lender adoption of mortgage modification programs. The aim of these policies was to stem the rising tide of foreclosures, especially in areas like California's Inland Empire that were acutely hit by the crisis. Yet despite the application of a unique policy to a highly salient housing market, there has been little focus on and no prior evaluation of California's crisis period policy efforts. In this paper, we undertake such an evaluation and use California as a laboratory to measure the effects of the California Foreclosure Prevention Laws (CFPLs).

California is a non-judicial foreclosure state. Prior to the CFPLs, the state required only that a lender or servicer (henceforth, lenders) initiating a home foreclosure deliver a notice of default (NOD; foreclosure start) to the borrower by mail. A 90-day waiting period then commenced before the lender could issue a notice of sale (NOS) of the property. In the midst of the housing crisis in July 2008, California passed the first of the CFPLs, Senate Bill 1137 (SB-1137).[3] This bill, which immediately went into effect, prohibited lenders from issuing an NOD until 30 days after informing the homeowner via telephone of foreclosure alternatives. The homeowner then had the right within 14 days to schedule a second meeting with the lender to discuss foreclosure alternatives. SB-1137 additionally mandated that agents who obtained a vacant residential property through foreclosure must maintain the property or face steep fines of up to $1000 per day. The following year in June 2009, California implemented the California Foreclosure Prevention Act (CFPA). The CFPA imposed an additional 90 day moratorium after NOD on lender conveyance of an NOS to borrowers unless the lender implemented a State-approved mortgage modification program. Together, the CFPLs (SB-1137 and the CFPA) significantly increased the lender pecuniary and time costs of home foreclosure. A full overview of the CFPLs is in appendix A.

[1] ACS Table-S1101 and Zillow.

[2] Mortgage Bankers' Association

[3] SB-1137 text.

The CFPLs were unique in scope and implemented at a moment when many California housing markets were spiraling downward. As such, these policies provide a rare opportunity to assess the housing impacts of important crisis-period policy interventions that sought to reduce foreclosures and encourage mortgage modification.

From the outset, the CFPLs were viewed with skepticism. In marked contrast to the California approach, the US Government elected not to increase foreclosure costs or durations during the crisis period. Indeed, Larry Summers and Tim Geithner, leading federal policymakers, argued that such increases would simply delay foreclosures until a later date.[4]

However, findings of recent academic studies suggest mechanisms whereby the CFPLs could have bolstered housing California housing markets. The key economic channel is based on the negative price impacts of foreclosure on the foreclosed home (Campbell et al., 2011) *and* neighborhood externalities, where foreclosures adversely affect nearby housing markets by increasing housing supply (Anenberg and Kung, 2014; Hartley, 2014) or through a "disamenity" effect where distressed homeowners neglect home maintenance (Gerardi et al., 2015; Lambie-Hanson, 2015; Cordell and Lambie-Hanson, 2016). More broadly, a spike in foreclosures lowers prices for the foreclosed and surrounding homes, which adversely affects local employment (Mian and Sufi, 2014), and finally the combination of employment and house prices losses leads to further foreclosures (Foote et al., 2008; Mian et al., 2015). By increasing lender foreclosure costs, the foregoing research thus suggests that the CFPLs may have slowed the downward cycle, mitigated the foreclosure externality, and buttressed ailing housing markets, especially in areas hard-hit by the crisis. Further, if the CFPLs reduced the adverse effects of the foreclosure externality at the height of the crisis, then the policy effects should be long lasting. These conjectures, however, have not been empirically tested, especially in response to a positive, policy-induced shock like the CFPLs.

Figure 1 presents motivating evidence regarding the impacts of the CFPLs via plots of housing indicators for California and the other Sand States. The blue dashed vertical lines indicate the implementations of SB-1137 and the CFPA. Data sources are in the figure notes. All Sand States behaved similarly prior to the CFPLs (e.g. the parallel pre-trends difference-in-differences assumption). Then with the passage of the CFPLs, California foreclosures and

[4] Summars (2014); Geithner (2010a).

mortgage default risk fell markedly and housing returns increased; these effects persisted through the end of the sample in 2014. In appendix B, we apply the Synthetic Control method to these indicators and show that following the implementation of the CFPLs that the improvement in the California housing market was exceptional compared to all other states.

Below we exploit more disaggregated data, within California and across state variation, and several estimation schemes to account for local housing and macro dynamics, loan-level characteristics, and California-specific macro trends in our identification of policy effects. We also emphasize our results surrounding the first of the CFPLs, SB-1137, which was implemented immediately upon passage in July 2008. This law change thus yields a unique opportunity for identification due its sharp timing and as it took effect early in the crisis before the announcement and implementation of the Federal Government's HAMP and HARP programs. Yet we document of the effects of the CFPLs on California housing over the entire evolution of the crisis.

Our findings suggest that the CFPLs were highly effective in stemming the crisis in California foreclosures. The CFPLs prevented 248,000 Real Estate Owned (REO; NOS) foreclosures, a reduction of 20.9%, and increased California *aggregate* housing returns by 6.2%. In doing so, they created \$350 billion of housing wealth. These effects were concentrated in areas most severely hit by the crisis. We further provide direct evidence that the CFPLs positively impacted housing markets using loan-level micro data: First we document that SB-1137 caused an increase in home maintenance and repair spending by lenders who took over foreclosed properties from defaulting borrowers, in line with the incentives of SB-1137 (recall that SB-1137 mandated that agents who took over foreclosed properties must maintain them or face fines of up to \$1000 per day). This increased maintenance and repair spending directly mitigates the foreclosure "disamenity" effect, a key reason why foreclosures create negative externalities.[5] As SB-1137 increased the cost of REO foreclosure via increased maintenance and repair spending and as longer REO foreclosure durations (e.g. the time from the lender takes over a foreclosed property to the time the property is disposed) are likely associated with higher maintenance costs, one may expect lenders to respond by reducing REO foreclosure

[5] See (Gerardi et al., 2015; Lambie-Hanson, 2015; Cordell and Lambie-Hanson, 2016).

duration. This is a key policy goal of a foreclosure mediation strategy (Geithner, 2010b) and what we find in our analysis of the policy, congruent with the CFPLs increasing foreclosure costs. In other direct evidence of the CFPLs impact, we also show that the CFPLs increased mortgage modifications. Specifically, we find that before the implementation of the Federal Government's HAMP and HARP programs that the CFPLs increased the mortgage modification rate by 20%. Overall, our results suggest that the CFPLs were a successful crisis-era intervention that required no pecuniary subsidies from taxpayers.

2 Data

We first estimate the effects of the CFPLs on the incidence of REO foreclosures using monthly Zillow REO foreclosures per 10,000 homes at the county level. We complement this data with controls and other variables compiled at the county level including Zillow house price returns; Land Unavailability as a predictor for house price growth (Lutz and Sand, 2017); Bartik (1991) labor demand shocks compiled from both the Census County Business Patterns (CBP) and the BLS Quarterly Census of Employment and Wages (QCEW); household income from the IRS Statistics of Income (SOI); the portion of subprime loan originated from HMDA data and HUD subprime originator list; and the non-occupied homeowner occupation rate as this may be a predictor of house price growth (Gao et al., 2017). We discuss these data in context below and list all data in appendix C.

We also assess the effects of the CFPLs using loan-level data from the Fannie Mae and Freddie Mac (GSE) loan performance datasets. While we have access to datasets that cover non-conforming loans (e.g. Corelogic or Blackbox), we use GSE loan performance data for two key reasons: First, the GSE data are publicly available, making our analysis transparent and re-producible. Second, and just as important, the GSEs apply similar standards across regions and do not discriminate based on geography (see Hurst et al. (2016) for a rigorous treatment), meaning that the set of GSE loans yields natural controls and treatment groups as regards the support of loan-level characteristics. We discuss our identification strategy for our loan-level analysis in depth below.

3 Estimation Methodology: CFPLs and County REO Foreclosures

We employ two separate estimation schemes to measure the effects of the CFPLs on foreclosures at the county level: The Synthetic Control method (Abadie et al., 2010, 2015) and a difference-in-difference-in-differences approach. Our other analyses (for example loan-level estimates) build on our approach described here; we discuss the differences in those sections.

Synthetic Control (Synth):

The Synth method generalizes the usual difference-in-differences, fixed effects estimator by allowing unobserved confounding factors to vary over time. For a given treated unit, Synth uses a data-driven algorithm to compute an optimal control from a weighted average of potential candidates not exposed to the treatment. The weights are chosen to best approximate the characteristics of the treated unit during pre-treatment period. For our foreclosure analysis, we iteratively construct a Synthetic Control Unit for each California county, where the characteristics used to build the Synthetic units are discussed below. The CFPL policy effect is the difference (Gap estimate) between each California county and its Synthetic. For inference, we conduct placebo experiments where we iteratively apply the treatment to each control unit. We retain the Gap estimate from each placebo experiment and construct bootstrapped confidence intervals for the null hypothesis of no policy effect (see also Acemoglu et al. (2016)). For California counties where Gap estimates extend beyond these confidence intervals, the CFPL effects are rare and large in magnitude.

Difference-In-Difference-In-Differences (DDD):

We also estimate the foreclosure impacts of the CFPLs through a DDD research design that exploits a predictive framework that measures ex ante expected variation in REO foreclosures within both California and across other states. Generally, the DDD approach allows us to control for California-specific macro trends while comparing high foreclosure areas in California to similar regions in other states (Imbens and Wooldridge, 2007; Wooldridge, 2011).

Our DDD specification for foreclosures is as follows:

$$
\begin{aligned}
\mathit{Forc/10K\ Homes}_{it} = & \sum_{\substack{y=1 \\ y \neq 2008\text{M}06}}^{T} \left(\theta_y \mathbf{1}\{y=t\} \times \mathit{HighForc}_i \times \mathit{CA}_i\right) \\
& + \sum_{\substack{y=1 \\ y \neq 2008\text{M}06}}^{T} \left(\mathbf{1}\{y=t\} \times \left(\beta_{1y} \mathit{HighForc}_i + \beta_{2y} \mathit{CA}_i + \mathbf{X}_i' \boldsymbol{\lambda}_y\right)\right) \\
& + \sum_{y=1}^{T} \mathbf{1}\{y=t\} \mathbf{X}_{it}' \boldsymbol{\gamma}_y \\
& + \delta_t + \delta_i + \varepsilon_{it}
\end{aligned} \tag{1}
$$

The dependent variable is Zillow REO foreclosures per 10K homes. *CA* and *HighForc* are indicators for California and high foreclosure counties, respectively. We define *HighForc* below. The excluded dummy for indicator and static variables is 2008M06, the month prior to the first CFPL announcement. The coefficients of interest are the interactions of monthly indicators with *CA* and *HighForc*, θ_y.

We employ a full set of time interactions to (i) examine the parallel pre-trends assumption; (ii) assess how quickly after implementation the CFPLs reduced REO foreclosures; and (iii) determine if there is any reversal in the CFPL policy effects towards the end of the sample.

Intuitively for each month y, θ_y is the difference-in-difference-in-differences in foreclosures where compare we ex ante "high foreclosure" counties to "low foreclosure" counties within California (first difference); then subtract off the difference between high and low foreclosure counties in other states (second difference); and finally evaluate this quantity relative to 2008M06 (third difference). The DDD estimates control for two potentially confounding trends: (i) changes in foreclosures of *HighForc* counties across states that are unrelated to the policy; and (ii) changes in California macro-level trends where identification of policy effects through θ_y assumes that the CFPLs have an outsized impact in *HighForc* counties.

The cumulative CFPL DDD policy estimate over the whole CFPL period is $\Theta = \sum_{y \geq 2008\text{M}07} \theta_y$, the total mean change in foreclosures for *HighForc* California counties. δ_t and δ_i are time and county fixed effects, and all regressions are weighted by the number of households in 2000. Controls (listed below) are fully interacted with the time indicators as their relationship with foreclosures may have changed during the crisis.

We also examine the robustness of the foregoing DDD approach by mimicking equation 1 with the Synth estimates and regressing the Synth Gaps on *HighForc* interacted with month indicators using only the California data in the final regression. This approach follows from the observation that Synth Gap estimates are generalized difference-in-differences estimates of California county-level foreclosures net of foreclosures in matched counties. The within California regression then provides the third difference. As the final regression uses a smaller California-only dataset, we retain county and time fixed effects but only interact the controls with a CFPL indicator.

To measure the county-level pre-CFPL expected exposure to foreclosures (*HighForc*), we forecast the increase (first-difference) in foreclosures (Δforeclosures) in each county for 2008Q3, the first CFPL treatment quarter, using only data up to 2008Q2 (pre-treatment data). A random forest (RF) model is used to build the forecasts as RF models often provide more accurate predictions than traditional techniques (Mullainathan and Spiess, 2017; Athey, 2018). We first train the RF model using data available up to 2008Q1 to predict Δforeclosures for 2008Q2. We then predict Δforeclosures for 2008Q3, the first CFPL treatment quarter, using data up to 2008Q2. Predictors used in our RF model include the levels and squared values of the first and second lags of Δforeclosures; the first and second lag of quarterly house price returns; the levels and squared 2007 unemployment rate; the interaction of the unemployment rate (or its square) and the house price returns as the combination of these quantities constitutes the double trigger theory of mortgage default (Foote et al., 2008); the percentage of subprime originations in 2005 (Mian and Sufi, 2009); Land Unavailability (Saiz, 2010; Lutz and Sand, 2017); an indicator for judicial foreclosure states (Mian et al., 2015); the 2005 non-owner occupied mortgage origination rate as a proxy of housing market speculation (Gao et al., 2017); and the maximum unemployment benefits for each county's state in 2007 (Hsu et al., 2018). Predictors also include 2007 income per household, a Sand State indicator, and pre-CFPL Bartik (1991) Labor Demand Shocks. We also interact the Bartik shocks with housing returns. Variable importance for each predictor in the RF model is plotted in appendix D.

To gauge predictive accuracy, we evaluate our RF predictions relative to traditional OLS models using the mean-squared error (MSE) for non-California counties in 2008Q3. The MSE for the RF model is 36.5% lower relative to a benchmark panel AR(2), indicating that the RF predictions are substantially more accurate. The MSE of the RF model is also 60.1% lower than a full OLS model that includes all aforementioned predictors.

We classify counties as either high or low foreclosure (*HighForc*) based on the RF predictions using a cross-validation approach. Specifically, we search from the US median predicted change in foreclosures for 2008Q3 (1.64 per 10K homes) to the 90th percentile (13.07 per 10K homes) and choose the cutoff for high foreclosure counties that minimizes the pre-treatment difference between the treatment and control groups in equation 1 (the cutoff that minimizes $\sum_{y<2008\text{M07}} \theta_y^2$). The cutoff chosen by the cross-validation procedure is 7.54 REO foreclosures per 10K homes, corresponding to the 82nd percentile, meaning that *HighForc* counties have a predicted increase in foreclosures of at least 7.54 per 10K homes for 2008Q3.

Note also that the RF model predicts marked foreclosure increases for the mean low foreclosure California county at 5.28 REO foreclosures per 10K homes for 2008Q3 (nearly five times the national median). Thus, there is room for foreclosures to fall in non-*HighForc* California counties and allow the DDD estimates to account for California macro-level trends that may lower foreclosures across the state.

The controls for the DDD model in equation 1 include the annual unemployment rate and Bartik shocks; 2008M01-2008M06 house price growth; Land Unavailability; the 2005 non-owner occupied mortgage origination rate; the 2005 subprime origination rate; and 2007 income per household.

4 The Impact of the CFPLs on County-Level Foreclosures

The estimates of the CFPL impacts on REO foreclosures using the Synth and DDD approaches are visualized in figure 2. The county-level attributes used to build the Synth matches for each California county use *only* pre-treatment data and include the following: RF predictions for Δforeclosures in 2008Q3, REO foreclosures, and variables used as controls in equation 1.

Panel 1A plots the cumulative Gap in REO foreclosures at various percentiles for California counties, where the percentiles are calculated within each month using only the California county-level Synth Gap estimates. The two blue-dashed vertical lines are the implementations of the SB-1137 and the CFPA, and the gray band is the 95% confidence interval bootstrapped from all placebo experiments associated with the null of no CFPL policy effect. Gap estimates that jut outside this confidence band are rare and large in magnitude.

During the pre-treatment period, the cumulative Gap is near zero across California percentiles, in line with the parallel pre-trends assumption. Then with passage of SB-1137 in 2008M07, REO foreclosures drop immediately for California counties at the 50th, 25th, and 10th percentiles. Counties at these percentiles are also bunched together towards the bottom end of the distribution below the 95% confidence interval; the distribution is thus right-skewed and a mass of California counties experienced a large and statistically significant CFPL drop in REO foreclosures. The decline in foreclosures for these counties continued through 2014, consistent with long lasting policy effects. Contrary to concerns expressed by federal policymakers, there is no evidence of reversal in aggregate county-level foreclosure trends. California counties at the 75th or 90th percentiles experienced comparatively little foreclosure mitigation. This latter finding is not surprising given the pre-CFPL heterogeneity across California housing markets.

The map in figure 2, panel 2 documents the geographic heterogeneity in CFPL foreclosure reduction. Specifically, panel 2 shows the Synth cumulative Gap in REO foreclosures from 2008M07-2011M12. Red areas represent a reduction in foreclosures relative to the Synth counterfactuals, gray areas indicate no change, blue areas correspond to an increase, and white areas have no data. Names are printed on the map for counties whose cumulative Gap is in the bottom 5th percentile relative to the empirical CDF of all placebo effects.

Overall, panel 2 shows that the areas most severely affected by the housing crisis also experienced the largest CFPL treatment effects, in line with the policy successfully targeting the most hard-hit regions. For example, San Bernardino and Riverside, lower income and supply elastic regions that constitute California's Inland Empire, were the epitome of the 2000s subprime crisis. These areas subsequently experienced large and beneficial CFPL policy effects: REO foreclosures per 10K homes in San Bernardino and Riverside fell by 428.25 (24.2%) and 379.74 (21.0%). Relative to the Synth counterfactuals, foreclosure reductions were also large in Los Angeles and central California, as well as in inland Northern California. Interestingly, we find no CPFL policy effects in California's wealthiest counties, located around the San Francisco Bay (Marin, San Mateo, Santa Clara, and San Francisco). Combining all of the Synth estimates across all California counties, results imply that the CFPLs prevented 248,000 REO foreclosures, a reduction of 20.9%.

Panel 1B of figure 2 plots the estimation output of θ_y from equation 1. The red line shows θ_y from a model that only includes time and county fixed effects (and the *CA* and *HighForc* indicators). The green line corresponds to the full model with controls. Shaded bands correspond to ± 2 standard error (SE) bands where robust SEs are clustered at the state level.

There are several key takeaways from panel 1B. First, the path of θ_y for the baseline and full models is similar, indicating that the estimates are robust to the inclusion of controls. Next, during the pre-treatment period, the ± 2 SE bands subsume the horizontal origin and thus the parallel pre-trends assumption is satisfied. Third and congruent with the foregoing Synth estimates, θ_y falls immediately after the implementation of SB-1137 in 2008M07. Note that HAMP and HARP, the federal mortgage modification programs, were announced in 2009M03 and not implemented in earnest until 2010M03.[6] Thus the CFPL policy effects in California substantially precede the announcement and implementation of the federal programs. Further, θ_y levels off at approximately -10 in January 2009 and remains at these levels until 2012, suggesting that the roll out of the federal programs did not change the path of θ_y. Fourth, there are no reversals in the CFPL policy effects as θ_y stays below the zero-axis through the end of the sample period, consistent with a mitigation of the foreclosure externality at the peak of the crisis having a long-lasting impact on REO foreclosure reduction. Finally, the total CFPL DDD estimate is $(\Theta = \sum_{y=2008M07}^{y=2011M12} \theta_y) = -451.44$ (Robust F-statistic: 20.60); meaning that for the average California *HighForc* county, the CFPLs reduced REO foreclosures by 451 per 10K homes. This estimate is in line with our above Synth results.

Last, panel 1C of figure 2 mimics equation 1 and panel 1B, but uses the Synth output and only within California data as discussed above to estimate θ_y. Hence, panel 1C documents the robustness of our results to an alternative, two-step estimation scheme. Overall, the path of the estimates in panel 1C closely matches panel 1B, but the magnitudes are slightly smaller. Specifically, θ_y in panel 1C hovers around the horizontal axis prior to 2008M07 in line with the parallel pre-trends assumption; falls immediately after the implementation of SB-1137; remains below the zero-axis and thus documents a reduction of foreclosures due to the CFPLs

[6] Agarwal et al. (2015, 2017) and their NBER working papers.

until 2012; and then returns to zero at the end of the sample period, implying no reversal in policy effects.

4.1 CFPL DDD REO Foreclosure Estimate Robustness and Falsification Tests

This sections further assesses the robustness of the regression results from equation 1 presented in panel 1B of figure 2. In particular, we first examine the parallel pre-trends assumption by including county linear and quadratic time trends. The model of interest now becomes

$$\begin{aligned} \textit{Forc/10K Homes}_{it} = & \sum_{\substack{y=1 \\ y\neq 2008M06}}^{T} (\theta_y \mathbf{1}\{y=t\} \times \textit{HighForc}_i \times \textit{CA}_i) \\ & + \sum_{\substack{y=1 \\ y\neq 2008M06}}^{T} (\mathbf{1}\{y=t\} \times (\beta_{1y}\textit{HighForc}_i + \beta_{2y}\textit{CA}_i + \mathbf{X}_i'\boldsymbol{\lambda}_y)) \\ & + \sum_{y=1}^{T} \mathbf{1}\{y=t\}\mathbf{X}_{it}'\boldsymbol{\gamma}_y \\ & + \delta_t + \delta_i + \sum_{i=1}^{N} \eta_i(\delta_i \times t) + \sum_{i=1}^{N} \zeta_i(\delta_i \times t^2) + \varepsilon_{it} \end{aligned} \tag{2}$$

where η_i and ζ_i are the coefficients on linear and quadratic county time trends for each of the $i = 1, \ldots, N$ counties. This model thus relaxes the pre-treatment common trends assumption. Equation 2 includes both linear and quadratic trends as foreclosures may have evolved non-linearly during the crisis. Note that the interpretation of coefficient of interest, θ_y, is somewhat different from equation 1. Here θ_y measures the deviation from common trends and thus the CFPL DDD effects will only be precisely estimated if the CFPLs induced a sharp reduction in foreclosures in high foreclosure California counties (relative to control regions) following the implementation of the CFPLs. In other words, these statistical tests will reveal if CFPLs created an immediate drop in REO foreclosures.

In total, the results are in panels A and B of figure 3. Panel A plots θ_y only when the regression model includes linear county time trends, while panel B employs both linear and quadratic county time trends. The path of θ_y in panel A is nearly identical to our previous estimates, providing further evidence that the parallel pre-trends assumption is satisfied and that the CFPLs created a large drop in REO foreclosures immediately following their introduction. In panel B where we include the both linear and quadratic time trends the estimates for remain statistically significant, again implying that the parallel pre-trends assumption is satisfied and that the CFPLs created a sharp drop in REO foreclosures for high foreclosure California counties. Note in panel B that the standard error bands are slightly wider as the inclusion of both linear and quadratic time trends reduces the degrees of freedom in the data.

The foregoing results show that CFPLs created a large and immediate drop in REO foreclosures following their implementation. These results are robust to various housing and macro controls, California macro trends, and region specific time trends. Further, the falsification tests executed within our Synthetic Control approach using non-California counties (e.g. distribution of these falsification tests is shown by the gray band in panel 1A of figure 2) show that the change in REO foreclosures following the CFPLs was unique to California relative to counties in all other states. Altogether, this evidence adds credence to the internal validity of our estimates and a causal interpretation of our results. While below we provide further evidence of the direct impact of the CFPLs at the loan-level, here we implement additional, important falsification and robustness tests using aggregated, county-level data. Indeed, the only remaining concern and threat to internal validity, from an aggregated data perspective, is that a separate positive shock had an outsized impact on high foreclosure California counties right as the CFPLs were implemented in July 2008 and this positive shock reduced foreclosures. We can explore the potential sources of these shocks by leaning on economic theory: From the double trigger theory of mortgage default (Foote et al., 2008), that says that households only default when they face negative equity *and* an adverse economic shock, we infer that only an outsized economic shock or house price shock can generate the effects like those documented above. We assess these shocks as potential confounders in turn.

First, we consider positive employment shocks. In our above estimates, we control Bartik labor demand shocks. As these labor demand shocks are exogenous to the local housing market (they are constructed through the interaction industry employment shares in 2000 and subsequent national growth), we include the Bartik shocks both before *and* after the implementation of the CFPLs above as controls. In other words, our foregoing estimates control for economic shocks to the local labor market during the pre-treatment and treatment periods. Here we further assess the role of employment shocks through a falsification test over the pre-treatment and treatment periods. In particular, we re-estimate our DDD regressions but let the dependent variable be BLS QCEW Bartik shocks (we eliminate the CBP Bartik shocks that were used above from our control set in this regression). If positive economic shocks are the cause of the observed reduction in REO foreclosures, the DDD estimates from this regression would be positive and large in magnitude. The results are in panel C of figure 3. The results show that (1) there were no differences in economic shocks across treatment and control groups during the pre-treatment period; (2) after the implementation of SB-1137 in July 2008, the treatment group of high foreclosure California counties did not experience positive, outsized economic shocks;[7] and (3) there were no outsized economic shocks following the implementation of the California Foreclosure Prevention Act in June 2009. These estimates therefore indicate that there were no positive employment shocks in high foreclosure California counties relative to controls were not the cause of the decline measured in our CFPL REO foreclosure DDD estimates.

Next, we examine the robustness of our results to changes in house prices directly *following* the implementation of the CFPLs. Note that our above estimates are robust to the inclusion Land Unavailability (a regional predictor of house price growth) and house price growth during the first half of 2008 (prior to the implementation of the CFPLs) as controls. Yet as stated above, if there was a large and positive house price shock at the same moment that the CFPLs were implemented, the portion of homeowners facing negative equity and subsequently foreclosures would decline. We address this concern by including an additional control, house price growth in the second half of 2008 (2008Q3 & 2008Q4). While including house price growth after the announcement and implementation of the CFPLs has the potential to be a "bad control" (Angrist and Pischke, 2008), the rational for including this control is that a reduction in foreclosures surfaces in house prices with a delay.[8] Yet even if positive 2008Q3/4 house price growth was caused by the foreclosure reduction associated with the CFPLs, its inclusion would simply bias our DDD CFPL foreclosure estimates towards zero. We present the results in figure 3 where the DDD estimates associated with the model shown in red only control for house price growth in the second half of 2008, while green line includes all of the aforementioned controls. Equation 1 remains our estimation equation and in both

[7]The red line, the model with no controls, suggests that the control group experienced a small, negative economic shock in January 2009. Only positive shocks are a threat to internal validity. Note also that once controls are included (green line) that this that the magnitude of the Bartik DDD estimate is substantially reduced.

[8]Due to, for example, illiquidity in the housing market, especially during this period.

models we interact house price growth in 2008Q3/4 with a full set of time dummies (less the excluded dummy) as in the second line of equation 1. The path of the CFPL REO foreclosure DDD estimates are in panel D of figure 3. These results match our previous findings and thus imply that house price changes at the moment of and immediately following the CFPL implementation are not a potential confound for our estimates of the impact of the CFPLs on foreclosures. Last, we note that the double theory of mortgage default specifies the interaction of negative house price growth and adverse economic shocks as the catalyst for foreclosure instantiation. Thus, we multiply 2008Q3/4 house price growth and Bartik shocks and use this interaction as a control. The results are in panel E of figure 3. The path of the DDD estimates matches our previous findings, meaning that the interaction of house price growth and labor demand shocks are not a confounder for our results.

5 CFPL DDD REO Foreclosure Loan-Level Estimates

One potential concern with our above analysis is that loan-level characteristics may differ across regions and thus contaminate our above results. While this is unlikely given the sharp reduction in foreclosures immediately following the introduction of the CFPLs, we address this concern here using GSE loan-level data. The key advantages of the GSE data are that (1) they are publicly available; and (2) the GSEs do not discriminate across regions, yielding loans that constitute natural control and treatment groups within a DDD analysis. Our outcome of interest is the probability that a mortgage enters REO foreclosure and we aim to estimate the DDD coefficients via a linear probability model that emulates equation 1. As shown below, our results after accounting for loan-level characteristics match above findings that employ county-level, aggregated data.

We proceed with estimation by employing a common two-step re-weighting technique (Borjas, 1987; Card, 2001; Altonji and Card, 1991)[9]. This approach allows us to recover the underlying micro, loan-level DDD estimates after controlling for loan-level characteristics, while accounting for the fact that REO foreclosure and loan disposition are absorbing states (e.g. once a loan enters REO foreclosure or is re-financed it is removed from the dataset) and thus that the number of loans available in each region during each time period may in itself depend on the treatment.

[9]For more recent references, see Angrist and Pischke (2008); Beaudry et al. (2012); Lutz et al. (2017).

In the first step we estimate the following loan-level regression, where noting that the lowest level of geographic aggregation in the GSE loan performance data are three digit zip codes:

$$\text{Prob(REO Forc)}_{it} = \sum_{y=1}^{T}\sum_{i=1}^{N}(\rho_{iy} \times \mathbf{1}\{y=t\} \times \text{zip3}_i) + \sum_{y=1}^{T}\sum_{i=1}^{N}(\mathbf{1}\{y=t\}\mathbf{X}_i'\boldsymbol{\tau}_{iy}) + e_{it} \quad (3)$$

The dependent variable is an indicator that takes a value of 1 for REO foreclosure and zero otherwise. ρ_{iy} are the year-month coefficients on zip3 $\times$ $\mathbf{1}\{y=t\}$ dummy variables and τ_{iy} are the coefficients on Loan $\times$ $\mathbf{1}\{y=t\}$ loan-level characteristics. Hence, we allow the impact of loan-level characteristics on the probability of REO foreclosure to vary flexibly with time as the predictive power of these characteristics may have changed with the evolution of the crisis. Broadly, equation 3 allows us to quality-adjust and thus purge our estimates from any bias associated with differences in loan-level characteristics. We estimate equation 3 using only loans originated during the pre-treatment period as loans originated subsequent to the CPFLs may have been affected by program treatment. Similarly, the vector of loan characteristics used as controls are only measured at loan origination as time-varying variables (such as current unpaid principal balance) may also be impacted by program treatment. $\mathbf{X}_i$ includes a wide array of loan characteristics which are listed in the notes to figure 4 that shows our final estimation output.

From the regression in equation 3 we retain the zip3-month coefficient estimates on the zip3 dummies, ρ_{iy}. In the second step of the estimation process, we employ the following model that yields the DDD estimates of the impact of the CFPLs on the probability of REO foreclosure using loan-level data (slightly changing the subscripts on ρ to match equation 1):

$$\begin{aligned}\rho_{it} = & \sum_{\substack{y=1 \\ y\neq 2008\text{M06}}}^{T} (\theta_y \mathbf{1}\{y=t\} \times \mathit{HighForc}_i \times \mathit{CA}_i) \\ & + \sum_{\substack{y=1 \\ y\neq 2008\text{M06}}}^{T} (\mathbf{1}\{y=t\} \times (\beta_{1y}\mathit{HighForc}_i + \beta_{2y}\mathit{CA}_i + \mathbf{X}_i'\boldsymbol{\lambda}_y)) \\ & + \mathbf{X}_{it}'\boldsymbol{\gamma}_y + \delta_t + \delta_i + \varepsilon_{it}\end{aligned} \quad (4)$$

θ_y is the DDD coefficient of interest and represents the impact of the CFPLs on loans in high foreclosure California zip3 regions after controlling for the change in the probability of foreclosure in low foreclosure California zip3 regions and the difference in the change in the foreclosure rate between high and low foreclosure zip3 regions in other states. We determine high foreclosure California zip3 regions based on the Random Forest predictions and process documented above. Aggregate controls include Land Unavailability as well as CBP and BLS QCEW Bartik labor demand shocks.

The results are in figure 4. The second-step regression in equation 4 is weighted by the number of households in 2000 and robust standard errors are clustered at the state level. The vertical axis in the plot is in basis points as the probability of REO foreclosure during a given month for a loan is quite small.

The path of θ_y in panel A figure 4 (both with and without extra macro and housing controls) is similar to our previous DDD estimates in figures 2 and 3, implying that our estimates of the impact of the CPFLs on REO foreclosures are robust the inclusion of loan-level characteristics as controls.

First, during the pre-treatment period, θ_y is a precisely estimated zero, indicating that the parallel pre-trends assumption is satisfied. Indeed, the F-statistic associated the DDD estimate with the cumulative probability that a loan enters REO foreclosure during the pre-treatment period ($\sum_{y<2008\text{M}07} \theta_y$) is 0.61 (p-value $= 0.44$).

Then with the announcement and implementation of the SB-1137 in July 2008, the first of the CFPLs, the probability of REO foreclosure for high foreclosure California zip3 regions falls immediately and sharply. The quick drop in the probability of REO foreclosure, even after controlling for loan-level characteristics and macro controls, buttresses the assertion that the reduction in high foreclosure California counties was due to the CFPLs: Before the announcement of HAMP in 2009M03, the probability that a mortgage in a high foreclosure California region succumbed to REO foreclosure ($\sum_{y=2008\text{M}07}^{2009\text{M}02} \theta_y$) fell by 19.29 basis points. Compared to the counterfactual of non-California high foreclosure regions where the pre-HAMP treatment period probability of REO foreclosure was 51.24 basis points (2008M07-2009M02); the DDD estimate represents a 38 percent decline in the REO foreclosure rate due to the CFPLs. The cluster-robust F-statistic associated with this DDD estimate during the pre-HAMP treatment period ($\sum_{y=2008\text{M}07}^{2009\text{M}02} \theta_y$) is 20.96 (p-value < 0.001), meaning that the reduction in the REO foreclosures following the implementation of SB-1137 and the introduction of the CFPLs was both large and statistically significant.

From there, θ_y stays below zero through 2011 as the CFPLs continued to reduce foreclosures in high foreclosure California regions over evolution of the crisis. θ_y then reverts back to zero (and becomes statistically insignificant) in late 2011 into 2012. Importantly, θ_y does not ascend above zero through the end of the sample period, in line with our above results that show that the CFPLs simply did not delay REO foreclosures until a later date.

Panel B of figure 4 controls for zip3 time trends and therefore assesses the parallel pre-trends assumption and if the CFPLs induced an immediate and sharp drop in the REO foreclosure rate. The path of θ_y is nearly identical across panels A and B of figure 4. Hence, the parallel pre-trends assumption appears to be satisfied as our results are robust to the inclusion of local housing market time trends.

Another possibility is that homes in high foreclosure California regions were are being disposed by a foreclosure alternative (Short Sale, Third Party Sale, Charge Off, or Note Sale). While foreclosure alternatives may reduce the number of empty homes in these regions, such resolutions would not have aided policymakers in their goal of keeping homeowners in their homes. We repeat the above analysis, but let the dependent variable be equal to 1 for mortgages that enter into a foreclosure alternate and zero otherwise. The path of the DDD coefficients is in appendix E. The results show that there was no change in incidence of foreclosure alternates during the early part of the crisis. Then, beginning in mid-2009, foreclosure alternates in high foreclosure California regions began to drop, meaning that the probability that a mortgage entered into a foreclosure alternative fell.

6 Foreclosure Maintenance and Repair Costs

In this section, we provide direct evidence that the CFPLs increased foreclosure costs by focusing on foreclosure maintenance and repair costs for homes in REO foreclosure. Recall that a key provision of SB-1137 was that agents who took over a home via REO foreclosure must maintain the home or face fines of to $1000 per property per day. This implies that policymakers believed that (1) homes obtained via REO foreclosure were not being properly maintained and (2) that the foreclosure externality "disamenity effect" was exacerbating the foreclosure crisis. Indeed, as noted in the introduction, previous research shows that "disamenity effects" are a key contributor to foreclosure externalities and thus limiting disamenity effects, by incentivizing home maintenance for example, will reduce foreclosures within a housing market. Further, increasing foreclosure costs changes the net-present-value calculation of foreclosure relative to modification.

From the GSE loan performance data, we retain all loans that enter REO foreclosure. For each REO foreclosure, the GSEs report the amount spent on maintenance and repairs for each home before disposition. The pre-treatment and CFPL treatment groups are based on the REO foreclosure date. For the pre-treatment group, we consider all homes that entered REO foreclosure before the announcement of the CFPLs *and* whose disposition date was also before the announcement of the CFPLs. REO foreclosures in the CFPL treatment period include only loans whose REO foreclosure date is after the announcement of SB-1137, but before the announcement of HAMP in 2009M03. Thus, these data include no loans that entered into REO foreclosure after the announcement of HAMP. Note that we drop all REO foreclosures where the REO foreclosure date is before SB-1137 but the disposition date is after SB-1137, as the GSEs only report total foreclosure costs and not foreclosure spending by month. With this data in hand, we estimate a DD regression where the dependent variable is foreclosure maintenance and repair costs:

$$\text{Forc Maintenance and Repair Costs}_{it} = \alpha + \gamma_i + \delta_t + \theta(\text{CA}_i \times \text{CFPL}_t) + \mathbf{X}_i'\boldsymbol{\lambda} + \varepsilon_{it} \qquad (5)$$

where the left-hand-side variable measures foreclosure maintenance and repair costs in dollars, γ_i is zip3 fixed effects, δ_t represents REO foreclosure date fixed effects, and the coefficient of interest, the DD estimate θ, captures the increase in foreclosure costs due to the SB-1137. Note that given our definition of the treatment and control groups (based on REO foreclosure date and disposition date), that the duration of time spent in foreclosure (and thus foreclosure costs) may vary with the REO foreclosure date. We account for this by including linear and quadratic effect effects in the months spent in REO foreclosure as well as REO foreclosure date fixed effects.

The results for non-judicial states are in table 1, those for all states are in appendix F. Column (1) of table 1 shows the results without any fixed effects or controls. Average foreclosure maintenance and repair costs for non-California properties during the pre-CFPL period was $3016.11. The coefficient on CA is near zero at $57.89 dollars with a standard error of \$270.29, implying that there were no average level differences in pre-treatment foreclosure spending across the treatment and controls groups and thus that the parallel pre-trends assumption is satisfied. This result is congruent with our expectations as the GSEs do not discriminate based on geography (Hurst et al., 2016). The coefficient on CFPL is \$478.73 and statistically significant, meaning that the during the CFPL period for non-California foreclosures that the GSEs spent nearly 16% more on average for maintenance and repairs than during the CFPL period. The coefficient on the CA $\times$ CFPL interaction, the DD estimate, is \$573.78 and statistically significant. This coefficient estimate suggests that on average that the increase in spending on foreclosure maintenance and repair doubled for California properties relative to non-California properties during the CFPL period.

Column (2) of table 1 adds linear and quadratic effects in the time spent in REO foreclosure. As expected, longer REO foreclosure durations correspond to higher maintenance spending. Yet the quadratic term is negative, suggesting that monthly spending falls as durations lengthen. This may due to the fixed costs associated with foreclosure maintenance or unwillingness of agents to spend on foreclosure maintenance at longer durations. Notice again that the coefficient on CA is insignificant, indicating that there are no level differences in pre-treatment foreclosure maintenance spending across treatment and control groups. Also, once we control for foreclosure durations, the coefficient on CFPL falls by half, but the coefficient on the CA $\times$ CFPL interaction only changes slightly. Comparing average foreclosure maintenance spending after accounting for foreclosure durations suggests that increase in foreclosure maintenance spending during the CFPL period was more than twice as high for California foreclosures relative to those in other states. Columns (3), (4), and (5) cumulatively add REO foreclosure date fixed effects, zip3 fixed effects, and loan-level controls respectively. The included loan-level controls are listed in the notes to table 1. The coefficient on the CA $\times$ CFPL interaction attenuates somewhat, but still remains large in magnitude and highly significant. Finally, columns (6) and (7) add linear and quadratic REO foreclosure date zip3 time trends. These tests allow us to assess the pre-trends assumption and the DD coefficients will only be precisely estimated if there is a sharp increase in foreclosure costs following the introduction of SB-1137. In columns (6) and (7) the DD coefficient is again large and magnitude and highly significant and thus implying that even after allowing for uncommon trends that there was a large and statistically significant increase in foreclosure maintenance and repair costs for California properties.

6.1 REO Foreclosure Durations

The above section documents that the CFPLs induced agents who took over homes via REO foreclosure to increase maintenance and repair spending. Further, if the extra maintenance spending comprised marginal costs associated with length of time in foreclosure (e.g. lawn maintenance for example), we would expect rational agents to circumvent these costs by disposing of homes obtained through REO foreclosure quicker. In other words, REO foreclosure durations may shorten. Indeed, shortening REO foreclosure durations is a key policy objective as empty homes contribute to the foreclosure "disamenity effect" and exacerbated the housing crisis (Geithner, 2010b).

Using a DD analysis, we assess the impact of the CFPLs REO foreclosure durations in table 2. Foreclosures are split into the pre-treatment and treatment groups as in section 6.[10] Columns (1) - (3) show the results for non-judicial states only, while columns (4) - (6) display the regression output where the dataset comprises all states. Loan-level controls match those from table 1 and robust standard error errors are clustered at the state level. Column (1) controls only for REO foreclosure date fixed effects (as the foreclosure durations vary with REO foreclosure date given how we split foreclosures into treatment and control groups). The middle panel shows that during the CFPL period, that average REO foreclosure duration for non-California properties in non-judicial states was 7.97 months. The coefficient on CA is near zero at 0.057 (less than one-tenth of a month) with a standard error of 0.301, indicating that there was no levels differences in average REO foreclosure durations during the pre-treatment period and thus that the parallel pre-trends assumption is satisfied. The coefficient on the CA $\times$ CFPL interaction is -0.662, indicating that foreclosure durations fell by over half a month for California properties. Yet as this coefficient is imprecisely estimated, it is not statistically significant at conventional levels. Columns (2) and (3) add zip3 fixed effects and loan controls, respectively. The coefficient on the CA $\times$ CFPL interaction with a full set of controls remains stable at -0.589, but its standard error falls markedly and therefore

[10]Note that the regressions in table 2 use more observations than those in table 1 because foreclosure and maintenance spending is missing for some REO foreclosures.

implying that the zip3 fixed effects and loan-level controls are uncorrelated with the CFPL treatment implementation in California but have predictive power for foreclosure durations. The DD coefficient in column (3) is statistically significant at conventional levels, indicating that the CFPLs shortened REO foreclosure durations.

Columns (4) - (6) show the results for all states. Overall the DD estimates are similar, but the standard errors are smaller as the sample size increases. This yields larger t-statistics. The coefficient on the CA $\times$ CFPL interaction in column (6), that includes all controls, is -0.475 with a standard error of 0.215. Congruent with our above results, this statistically significant DD estimate means that the CFPLs shortened foreclosure durations by just under a half of a month during the CPFL period.

7 Mortgage Modifications

While the overarching aim of the CFPLs was to reduce foreclosures, the policy also sought to increase modifications. This sections uses GSE loan-level data to assess the change in the modification rate due to the CFPLs. We employ the same two-step estimation procedure described above in section 5, but in this case the outcome variable of interest is the probability of loan modification. Step 1 of the two step procedure is identical to that described in section 5, but we use an indicator for mortgage modification as the left-hand-side variable. In the second step, we estimate the following DD regression:

$$\rho_{it} = \sum_{\substack{y=1 \\ y\neq 2008M06}}^{T} (\mathbf{1}\{y=t\} \times (\theta_y CA_i + \mathbf{X}_i'\boldsymbol{\lambda}_y)) + \mathbf{X}_{it}'\boldsymbol{\gamma}_y + \delta_t + \delta_i + \varepsilon_{it} \tag{6}$$

where ρ_{it} are the coefficient estimates on zip3 $\times$ time dummy variables from the first step of the procedure that control for loan-level characteristics. The coefficient of interest is θ_y that measures the difference-in-differences in the probability of loan modification in California relative to other states. δ_i and δ_t are zip3 and year-month fixed effects and the static and time varying controls include zip3 land unavailability as well as CBP and BLS QCEW Bartik shocks, respectively. The regression is weighted by the number of households in 2000 and robust standard errors are clustered at the state level.

The DD regression here is of interest as θ_y measures, after controlling for loan-level characteristics, the change in the probability of mortgage modification induced by the CFPLs in

California.

We plot the estimation output of θ_y from the above equation in panel A of figure 5. The vertical axis is in basis points. The path of θ_y shows that there is no pre-treatment difference in the modification rate prior to the CFPLs, meaning that the parallel pre-trends assumption is satisfied (to the left of the first blue-dashed vertical line). Then with the passage of SB-1137 in July 2008, we see a statistically significant increase in the modification rate. Recall that HAMP and HARP were not announced until March 2009 (and not implemented until March 2010 (Agarwal et al., 2015, 2017)). Thus prior to the announcement of the Federal programs, the CFPLs caused an increase in the modification rate of 4.05 basis points ($\sum_{y=2008M07}^{2008M03} \theta_y$). While this may seem small, note that the modification rate in general is small. For the counterfactual non-California regions during the pre-HAMP treatment period, the probability of mortgage modification was just 10.59 basis points. Together, these estimates imply that the SB-1137 increased the modification rate by a 38 percent.

Then following the implementation of the California Foreclosure Prevention Act in June 2009, the modification rate increased markedly. Note that the rollout of HAMP and HARP did not begin until March 2010 (Agarwal et al., 2015, 2017) and thus the increase in modifications due to the CFPA proceeded the implementations of the Federal programs. From the announcement of the CFPLs in July 2008 to February 2010, just prior the implementation of HAMP and HARP ($\sum_{y=2008M07}^{2008M03} \theta_y$) was 22.56 basis points. The associated F-statistic is 13.74 (p-value < 0.001). Thus, the increase in modifications induced by the CFPLs was large in magnitude and statistically significant. Using data through end the end of 2012, the estimated increase in the modification rate due the CFPLs is 130.89 basis. A back of the envelope application of this estimate applied to all of California mortgages during the CFPL period suggests that CFPLs led to an additional 70,000 mortgage modifications, without requiring any pecuniary subsidies from taxpayers. In contrast, nationwide HAMP subsidized both lenders and borrowers but led to just 1 million modifications (Agarwal et al., 2017). Using the total number of housing units with a mortgage from the ACS Survey, the above estimates imply that the CFPLs induced 68 percent of the modification increases relative to HAMP without any pecuniary subsidies.[11] HAMP also did not include any provisions to

[11]Using the estimate that HAMP created 1 million modifications from Agarwal et al. (2017) and data from table B25081 from the 1-year 2007 ACS survey, suggests that the modification rate for HAMP was

increase foreclosure costs like the CFPLs.

Panel B controls for zip3 time-trends. The estimates match our above findings, implying that the parallel pre-trends assumption is satisfied and that CFPLs led to an deviation in the modification rate relative to local trends.

8 CFPL Foreclosure Reduction and House Price Growth

Extant research suggests that foreclosures reduce prices for foreclosed homes *and* neighboring homes through a supply response or a "disamenity" effect. Indeed, an extensive literature aims to estimate the effects of foreclosures on house prices, but none do so in response to a positive policy induced shock (foreclosure mitigation) during a crisis.[12] Previous studies also largely focus on neighborhood effects, while our analysis benefits from a large-scale policy experiment in the nation's largest housing market. We thus contribute to the literature by measuring the causal impact of CFPLs on house prices and estimating aggregate price effects in response to foreclosure reduction. These findings also provide insight as to the spatial impact of mortgage defaults and foreclosure mitigation policies.

We estimate the house price impacts of CFPL foreclosure alleviation through a three-step approach that mimics a DDD design. First, we retain our Synth REO foreclosure Gap estimates (figure 2, panel 2), the difference-in-differences in foreclosures for each California county relative to their Synth counterfactuals.

Our dependent variable is CFPL house price growth at the zip code level. Clearly, California house prices may change for reasons unrelated to the CFPLs (such as broader housing recovery). Thus, we next obtain the abnormal house price growth for each California zip code – analogous to an abnormal equity return – through Synth Gap estimates.[13] For each California zip code, we apply the Synth method and retain the Gap estimate for house price growth during the CFPL period.

We plot the median CFPL house price growth Gap estimate within each California county in figure 6, panel 1. The notes to figure 6 list the variables used to build the zip code Synthetic

1,000,000/51,962,570 = 0.019. In comparison the modification rate computed above for the CFPLs was 0.013. Thus 0.013/0.019 = 68.4. The number of California housing units with a mortgage from that same ACS survey is 5,381,874. Thus 5,381,874 * 0.013089 = 70,443.35 modified California mortgages.

[12] Campbell et al. (2011); Anenberg and Kung (2014); Gerardi et al. (2015); Fisher et al. (2015); Mian et al. (2015).

[13] Abnormal Return = Actual Return − Expected Return

counterfactuals. The county names printed on the map are from figure 2. Generally, in counties where the CFPLs lowered foreclosures, like San Bernardino, house prices increased.

We test this visual anecdote more formally as the third step in our estimation scheme in figure 6, panel 2A. Here we regress the Gap in CFPL house price growth on the Gap in CFPL REO foreclosures within California (weighted by the number of households in 2000). County foreclosure Gap estimates are mapped to zip codes using the Missouri Data Bridge. The slope estimates in panel 2A are DDD CFPL estimates that measure the increase in house prices due to a decline in foreclosures. Using OLS, the slope is -0.029 (robust SE clustered at the three-digit zip code level: 0.009), while the median slope from a quantile regression that is robust to outliers is -0.033 (robust SE: 0.003). Appendix G shows the point estimates from panel 2A, and re-estimates these regressions controlling for the 2009-2011 Bartik shock as well as 2007 household income and levels house prices, proxies of zip code income and housing wealth. The estimates are similar.

Using the median slope estimate (-0.033) and the median CFPL Synth Gap decline in REO foreclosures per 10K homes (-303.48), CFPL REO foreclosure reduction increased housing returns for the median zip code by 10.02%. Applying the distribution of REO foreclosure quantile regression estimates across California implies that the CFPLs increased California *aggregate* house price returns by 6.2% ($350 billion).

Finally, figure 6, panel 2B assesses shows mean abnormal house price growth for CFPL REO foreclosure reduction quintiles. The plot shows that the impact of CFPL REO foreclosure reduction on house prices is concentrated in cases where there was large, but not extremely large, REO foreclosure reduction. For counties in the second quintile in terms of CFPL REO foreclosure reduction, abnormal house prices increased 13 percent. In areas with the minimal foreclosure reduction (e.g. quintiles 4 and 5), there was little abnormal house price changes.

9 Did the CFPLs Create Adverse Side Effects for New Borrowers?

The CFPLs increased the lender foreclosure costs and thus ex post, may have reduced the value of the lender foreclosure option. As noted by Alston (1984), if the value of the foreclosure option declines, lenders may respond by either (1) increasing interest rates on new mortgages to compensate for the depreciation of the foreclosure option; or (2) rationing credit, especially in environments where raising interest rates is infeasible.[14] For the CFPLs, (1) would translate into fewer loans being originated in California post-policy, *ceteris paribus*. With regard to (2), Alston notes that during the Depression, lenders were reluctant to increase interest rates as this would have created "hostility and ill will" (p. 451). Similar concerns may have also deterred lenders from increasing interest rates in California following housing crisis.

Conversely, in their report on the CFPA, California (2010) notes that the number of applications for an exemption from the CFPA foreclosure moratorium was lower than anticipated, suggesting that the lender value of the foreclosure option was limited given the depths of the crisis. Also if the CFPLs aided depressed California housing markets (as documented above), then lenders may have viewed the CFPLs favorably as foreclosures can create dead weight losses for lenders (Bolton and Rosenthal, 2002).

We employ the HMDA dataset to determine the impact of the CFPLs on mortgage credit following the implementation of the policy. We only consider loans not sold to GSEs as GSEs do not discriminate based on region (Hurst et al., 2016). The results are in table 3. First, we use loan-level data to determine whether the probability of being denied a mortgage is higher in California, in line with a credit rationing response for new borrowers following the CFPLs. Specifically, we consider a linear probability model where the dependent variable is an indicator that equals one for mortgage loan denial.[15] The key independent variable is an indicator for mortgages originated in California. Controls are listed in the notes to table 3 and the data range from 2009 to 2014. We first restrict the dataset to Arizona, California, Florida, and Nevada (column (1)), as the housing dynamics of these states were similar during the 2000s; for robustness we also consider a dataset with California, Colorado, New York, and Texas (column (2)), states that were less affected by and rebounded quickly from the crisis. Robust standard errors are clustered at the 3-digit zip code level. A positive coefficient on *California* would suggest that Californians were more likely to be denied mortgage credit. If anything, the results in columns (1) and (2) show opposite: The probability of denial in post-CFPL California was slightly lower. Hence, Californians were no more likely than residents in the other states to be denied mortgage credit in the wake of the CFPLs.

Columns (3) - (6) examine loan volume growth following the implementation of the CFPLs.

[14] Lenders ration credit as underwriting costs increase (Sharpe and Sherlund, 2016).

[15] We do not know if denied mortgages would have eventually been sold.

We consider loan growth at the zip code level, both in terms of the number and dollar volume of loans, for 2009 through 2014 relative to 2007 using only loans not sold to GSEs. The key independent variable is an indicator for California and robust standard errors are clustered at the commuting zone level. Here, if mortgage lenders were rationing credit to California zip codes, relative to those in other states, the coefficient on *California* would be negative. Again, we find the opposite effect. The estimates imply that loan volume growth was instead higher in California zip codes. In total, the results in table 3 show that new California borrowers were not adversely affected by the CFPLs.

10 Discussion

Our above Synth results show that the CFPLs prevented 248,000 REO foreclosures in California. Our estimated effects are large in magnitude relative to other federal government programs. *Outside* of California, HAMP and HARP, the federal mortgage modification programs prevented approximately 230,000 and 80,000 REO foreclosures respectively (Agarwal et al., 2015, 2017).[16] Also note that HAMP and HARP are not a threat to identification as the CFPL effects preceded the announcement and implementation of the federal programs (figure 2). Similarly outside of California, Hsu et al. (2018) find that unemployment insurance prevented 500,000 REO foreclosures. Hence relative to these other programs, the impact of the CFPLs on foreclosures is large in magnitude. The CFPLs were also relatively costless compared to these other programs as they did not provide pecuniary subsidies to lenders and borrowers (HAMP/HARP) or to unemployed households (unemployment insurance).

A potential concern not previously addressed is unemployment insurance as a confounding factor. Although our DDD analysis controls for California-level trends, we explicitly account for unemployment insurance here by re-estimating our Synth foreclosure results using only states within the same quintile as California in terms of crisis-era unemployment benefits based on data from Hsu et al. (2018). Using this alternate control group, the CFPLs lowered REO foreclosures by 426,000 (31.3%), making our above results conservative in nature.

Our above Synth analysis also employs judicial and non-judicial states in the control group, whereas California is a non-judicial foreclosure state. Re-estimating our Synth results using only non-judicial states in the control group suggests that the CFPLs reduced foreclosures by

[16]Numbers from Hsu et al. (2018) and the Mortgage Bankers' Association.

20.4%, in line with our above estimates.

Other states also proposed legislation to impose foreclosure moratoria, but to our knowledge none of these proposed bills matched the breadth of the CFPLs and most were not enacted.[17] One state-level intervention did occur in Massachusetts, who passed a foreclosure right-to-cure law in 2008. Gerardi et al. (2013) find that the law did not improve borrower foreclosure outcomes. Unlike the Massachusetts law, the CFPLs were larger in scope and implemented when California housing markets faced extreme distress. Our results are robust to the exclusion of Massachusetts as a control. Yet the inclusion of Massachusetts does not pose a threat to identification as positive effects owed to the Massachusetts law would bias our results towards zero.

10.1 External Validity

While the aim of this paper is to establish internal validity for estimates of the impact of the CFPLs on California, external validity (e.g. other instances where similar policies were implemented) is of interest as well. We discuss external validity in the context of other research. One noteworthy instance of external validity arises from the Great Depression and the study of farm foreclosure moratoria. This analysis was carried out by Rucker and Alston (1987). Congruent with the our analysis of the CFPLs during the recent crisis, Rucker and Alston find that the farm foreclosure moratoria reduced farm foreclosure during the Great Depression. Further, Mian et al. (2015) study judicial and non-judicial states during crisis and conclude that the increased costs associated with judicial foreclosure limited foreclosure instantiation. Note that, however, there is debate regarding robustness of the impacts of judicial foreclosure (Gerardi et al., 2013). While the CFPLs were similar in some aspects to the aforementioned policies, they were unique in their scope and implementation: The CFPLs directly incentivized foreclosure maintenance spending, while *also* encouraging modifications through foreclosure moratoria. Overall, the efficacy of the CFPLs matches the extant research on foreclosures, while Rucker and Alston document that moratoria, a portion of the CFPL response, provided foreclosure relief during the Great Depression.

[17] See 2008-2009 proposed (but not enacted) legislation: Connecticut, Massachusetts (link1, link2, link3), Michigan (link1, link2, link3), Minnesota, South Carolina (link1, link2), Illinois, and Arkansas. Nevada implemented a foreclosure mediation program in 2009M07.

11 Conclusion

In this paper, we estimate the impacts of the California Foreclosure Prevention Laws. Our results show that the CFPLs prevented 248,000 REO foreclosures and created \$350 billion in housing wealth. These results are large in magnitude, economically meaningful, and show how the CFPLs, a foreclosure intervention that did not require any pecuniary subsidies, boosted ailing housing markets. A back of the envelope application of our estimates to non-California, high foreclosure counties indicates that the implementation of the CFPLs in these counties would have prevented an additional 104,000 REO foreclosures and created \$71 billion in housing wealth.

References

A. Abadie, A. Diamond, and J. Hainmueller. Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, 105(490), 2010.

A. Abadie, A. Diamond, and J. Hainmueller. Comparative politics and the synthetic control method. *American Journal of Political Science*, 59(2):495–510, 2015.

D. Acemoglu, S. Johnson, A. Kermani, J. Kwak, and T. Mitton. The value of connections in turbulent times: Evidence from the United States. *Journal of Financial Economics*, 121 (2):368–391, 2016.

S. Agarwal, G. Amromin, S. Chomsisengphet, T. Piskorski, A. Seru, and V. Yao. Mortgage refinancing, consumer spending, and competition: Evidence from the home affordable refinancing program. Technical report, National Bureau of Economic Research, 2015.

S. Agarwal, G. Amromin, I. Ben-David, S. Chomsisengphet, T. Piskorski, and A. Seru. Policy intervention in debt renegotiation: Evidence from the home affordable modification program. *Journal of Political Economy*, 125(3):654–712, 2017.

L. J. Alston. Farm foreclosure moratorium legislation: A lesson from the past. *The American Economic Review*, 74(3):445–457, 1984.

J. G. Altonji and D. Card. The effects of immigration on the labor market outcomes of less-skilled natives. In *Immigration, trade, and the labor market*, pages 201–234. University of Chicago Press, 1991.

E. Anenberg and E. Kung. Estimates of the size and source of price declines due to nearby foreclosures. *American Economic Review*, 104(8):2527–51, 2014.

J. D. Angrist and J.-S. Pischke. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, 2008.

S. Athey. The impact of machine learning on economics. *Working Paper*, 2018.

T. J. Bartik. *Who Benefits from State and Local Economic Development Policies?* Books from Upjohn Press. W.E. Upjohn Institute for Employment Research, November 1991.

P. Beaudry, D. A. Green, and B. Sand. Does industrial composition matter for wages? a test of search and bargaining theory. *Econometrica*, 80(3):1063–1104, 2012.

P. Bolton and H. Rosenthal. Political intervention in debt contracts. *Journal of Political Economy*, 110(5):1103–1134, 2002.

G. J. Borjas. Immigrants, minorities, and labor market competition. *ILR Review*, 40(3): 382–392, 1987.

California. California foreclosure prevention act report. Technical report, California Department of Corporations, 2010.

J. Y. Campbell, S. Giglio, and P. Pathak. Forced sales and house prices. *American Economic Review*, 101(5):2108–31, 2011.

D. Card. Immigrant inflows, native outflows, and the local labor market impacts of higher immigration. *Journal of Labor Economics*, 19(1):22–64, 2001.

M. Chauvet, S. Gabriel, and C. Lutz. Mortgage default risk: New evidence from internet search queries. *Journal of Urban Economics*, 96:91–111, 2016.

L. Cordell and L. Lambie-Hanson. A cost-benefit analysis of judicial foreclosure delay and a preliminary look at new mortgage servicing rules. *Journal of Economics and Business*, 84: 30–49, 2016.

L. M. Fisher, L. Lambie-Hanson, and P. Willen. The role of proximity in foreclosure externalities: Evidence from condominiums. *American Economic Journal: Economic Policy*, 7 (1):119–40, 2015.

C. L. Foote, K. Gerardi, and P. S. Willen. Negative equity and foreclosure: Theory and evidence. *Journal of Urban Economics*, 64(2):234–245, 2008.

Z. Gao, M. Sockin, and W. Xiong. Economic consequences of housing speculation. *Working paper*, 2017.

T. Geithner. Geithner calls foreclosure moratorium 'very damaging', 2010a. Online; posted 10-October-2010.

T. Geithner. Tim geithner interview with charlie rose, 2010b. Online; posted 10-October-2010.

K. Gerardi, L. Lambie-Hanson, and P. S. Willen. Do borrower rights improve borrower outcomes? evidence from the foreclosure process. *Journal of Urban Economics*, 73(1):1–17, 2013.

K. Gerardi, E. Rosenblatt, P. S. Willen, and V. Yao. Foreclosure externalities: New evidence. *Journal of Urban Economics*, 87:42–56, 2015.

D. Hartley. The effect of foreclosures on nearby housing prices: Supply or dis-amenity? *Regional Science and Urban Economics*, 49:108–117, 2014.

J. W. Hsu, D. A. Matsa, and B. T. Melzer. Unemployment insurance as a housing market stabilizer. *American Economic Review*, 108(1):49–81, 2018.

E. Hurst, B. J. Keys, A. Seru, and J. Vavra. Regional redistribution through the us mortgage market. *The American Economic Review*, 106(10):2982–3028, 2016.

G. Imbens and J. Wooldridge. Difference-in-differences estimation, 2007.

R. Koenker and K. F. Hallock. Quantile regression. *Journal of economic perspectives*, 15(4): 143–156, 2001.

L. Lambie-Hanson. When does delinquency result in neglect? mortgage distress and property maintenance. *Journal of Urban Economics*, 90:1–16, 2015.

C. Lutz and B. Sand. Highly disaggregated land unavailability. *Working Paper*, 2017.

C. Lutz, A. Rzeznik, and B. Sand. Local economic conditions and local equity preferences: Evidence from mutual funds during the us housing boom and bust. 2017.

A. Mian and A. Sufi. The consequences of mortgage credit expansion: Evidence from the us mortgage default crisis. *The Quarterly Journal of Economics*, 124(4):1449–1496, 2009.

A. Mian and A. Sufi. What explains the 2007–2009 drop in employment? *Econometrica*, 82 (6):2197–2223, 2014.

A. Mian, A. Sufi, and F. Trebbi. Foreclosures, house prices, and the real economy. *The Journal of Finance*, 70(6):2587–2634, 2015.

S. Mullainathan and J. Spiess. Machine learning: an applied econometric approach. *Journal of Economic Perspectives*, 31(2):87–106, 2017.

R. R. Rucker and L. J. Alston. Farm failures and government intervention: A case study of the 1930's. *The American Economic Review*, 77(4):724–730, 1987.

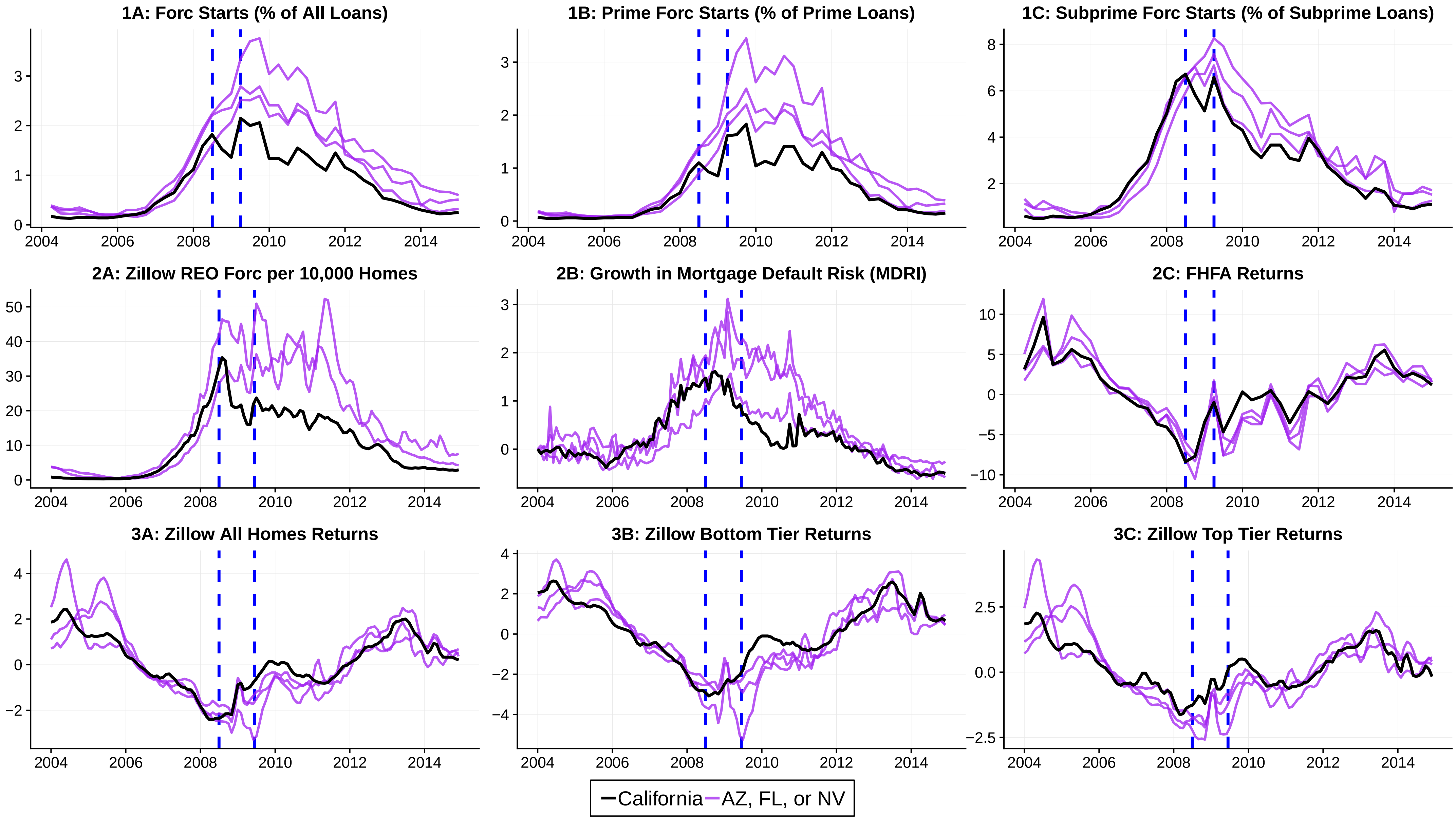
A. Saiz. The geographic determinants of housing supply. *Quarterly Journal of Economics*, 125(3), 2010.

S. A. Sharpe and S. M. Sherlund. Crowding out effects of refinancing on new purchase mortgages. *Review of Industrial Organization*, 48(2):209–239, 2016.

L. Summars. Lawrence Summers on 'House of Debt', 2014. Online; posted 6-June-2014.

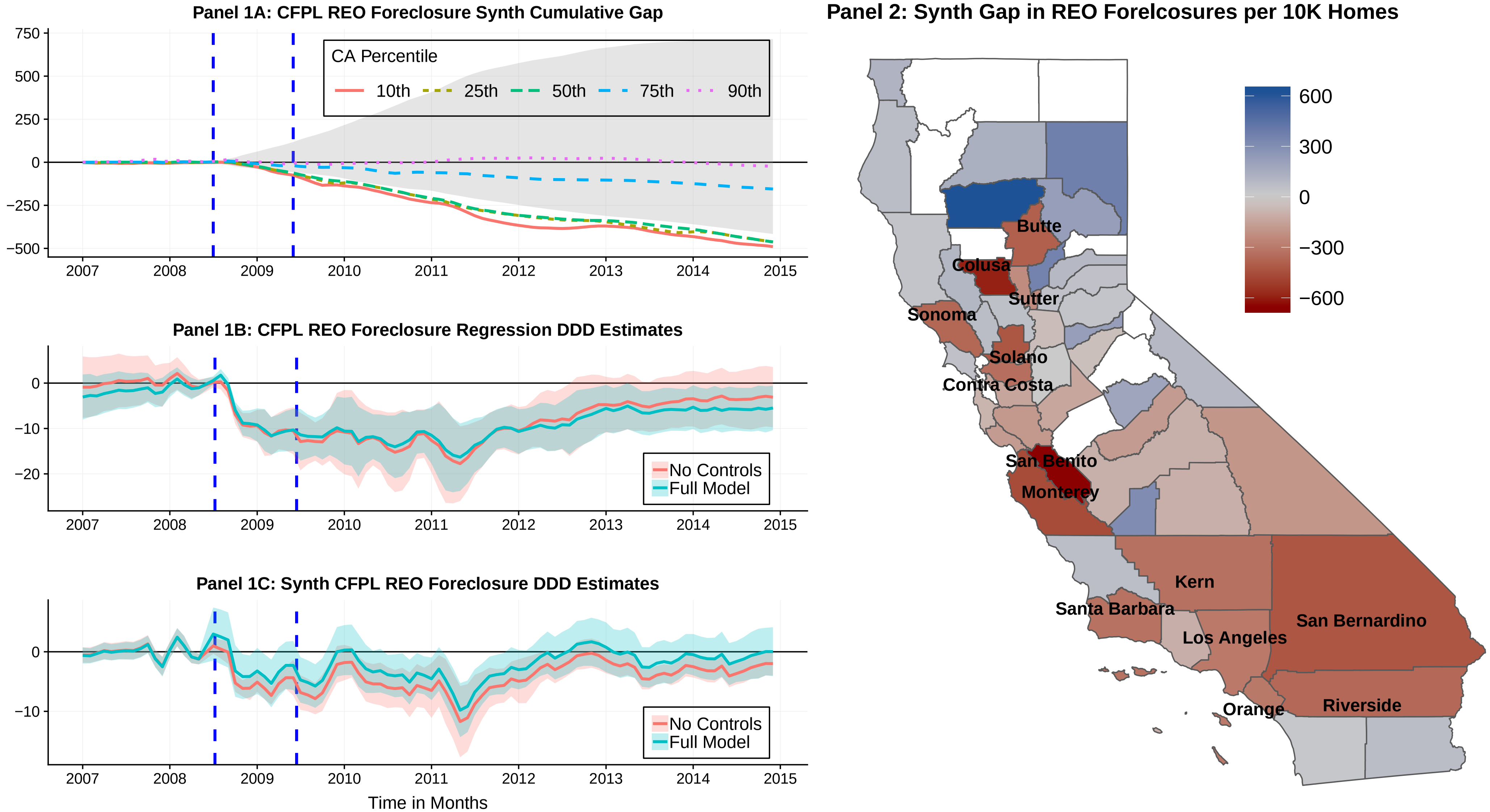
J. Wooldridge. Difference-in-differences estimation. http://www.eief.it/files/2011/10/slides_6_diffindiffs.pdf, 2011.

Figure 1: Sand State Foreclosures, Mortgage Distress, and Housing Returns



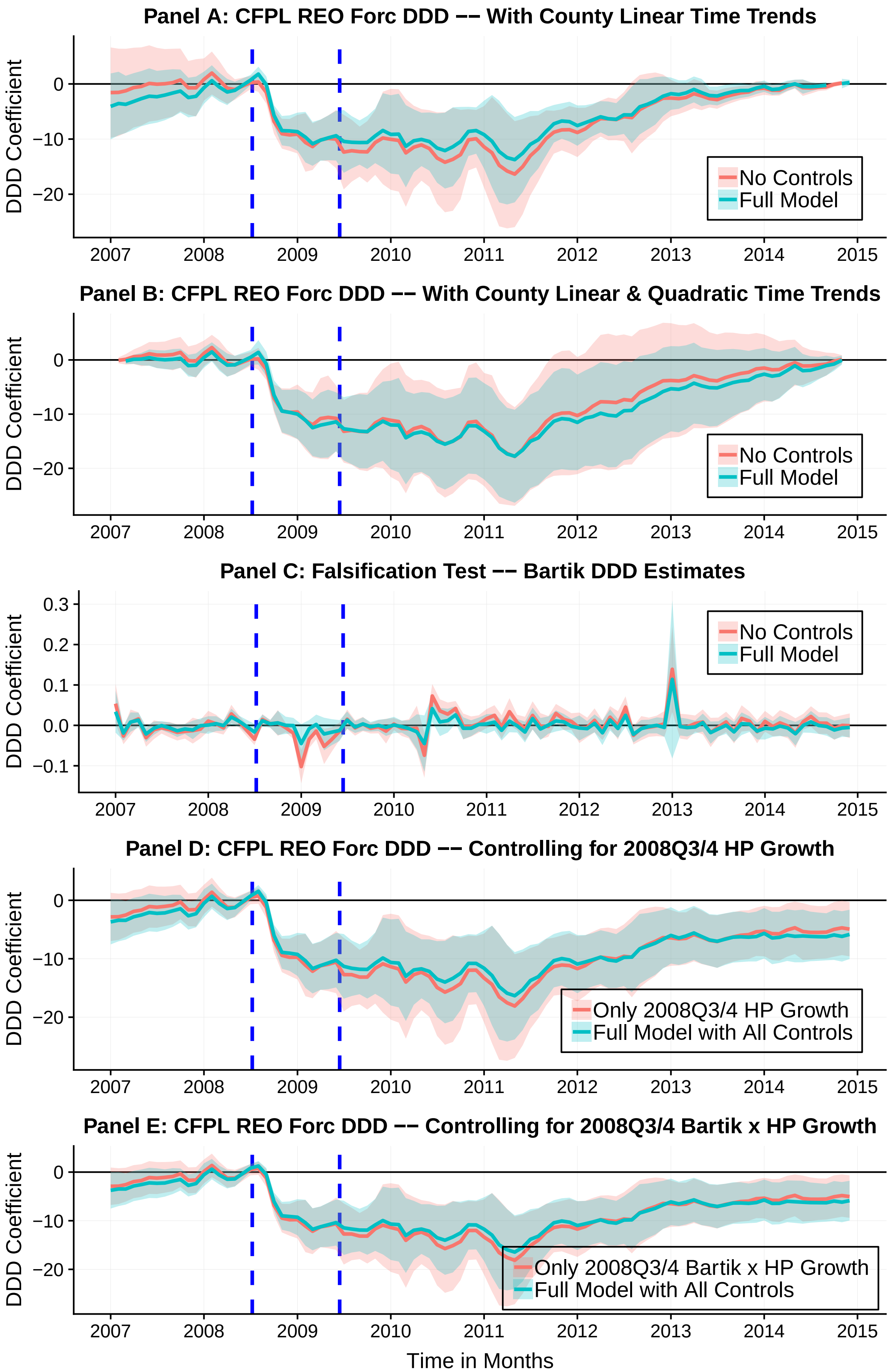
Notes: Plots of foreclosures, mortgage distress, and housing returns for Arizona, California, Florida, and Nevada. The black line is California, the purple lines represent Arizona, Florida, or Nevada. The first dashed-blue vertical line signifies the passage SB-1137 in 2008Q3 (2008M07); and the second dashed-blue vertical line represents the CFPA implementation date in 2009Q2 (2009M06). Foreclosure starts are from the Mortgage Bankers' Association, REO foreclosures are from Zillow (Note: Zillow does not report REO foreclosures for Florida); the Mortgage Default Risk Index (MDRI) is from Chauvet et al. (2016); and housing returns are from the FHFA and Zillow. See the data list in appendix C for more information on data sources.

Figure 2: County Level CFPL REO Foreclosure Estimates



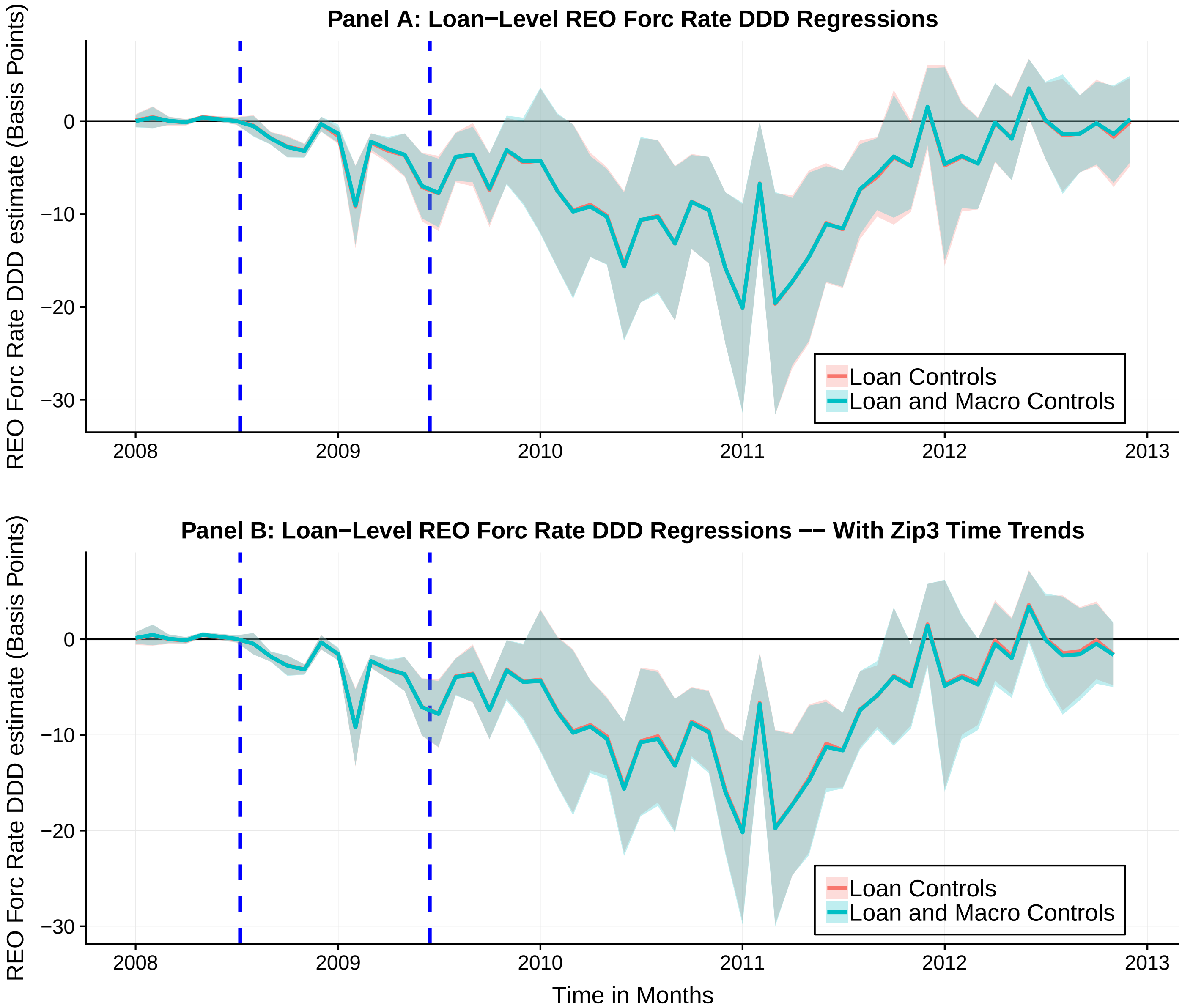
Notes: Panel 1A shows the Synth cumulative Gap in county-level REO foreclosures per 10K homes for California counties at the 10th, 25th, 50th, 75th, and 90th percentiles from 2007M01 to 2014M12. The two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA in 2008M07 and 2009M06 respectively. The gray band represents a 95 percent bootstrapped confidence interval estimated from all placebo experiments corresponding to the null hypothesis of no CFPL policy effects. Panel 2 shows the cumulative Gap in REO foreclosures per 10K homes from 2008M07-2011M12 across California counties. Counties in white have no data. County names are printed on the map if their cumulative Gap in REO foreclosures per 10K homes is in the bottom 5th percentile relative to the empirical CDF of all estimated placebo effects. Panel 1B shows the estimates of θ_y from equation 1 where the bands are ± 2 standard error bands based on robust standard errors clustered at the state level. All regressions are weighted by the number of households in 2000. The model with no controls only employs the *CA* and *HighForc* indicators as well as county and time fixed effects; the full model uses county and time fixed effects as well controls fully interacted with the time indicators. Panel 1C is the implementation of equation 1 using the Synth output. In panel 1C ± 2 standard error bands are computed using robust standard errors clustered at the county level.

Figure 3: County Level CFPL REO Foreclosure Robustness and Falsification Tests



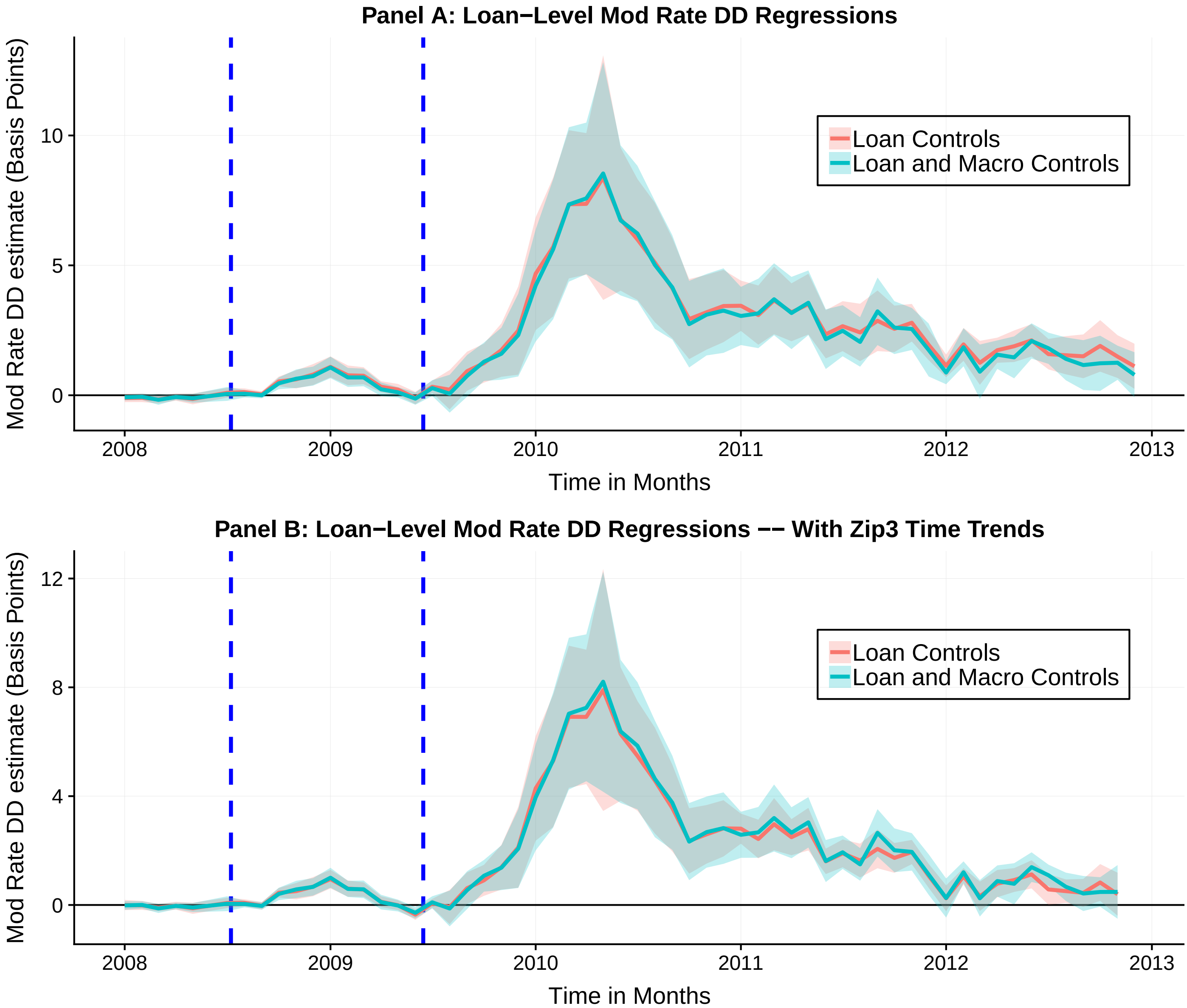
Notes: Robustness and Falsification tests for θ_y from equation 1. Panel A re-estimates equation 1 with zip3 linear time trends (zip3 dummies $\times$ time trend), while panel B employs both linear and quadratic zip3 time trends. Panel C presents a falsification test estimated using the setup in equation 1 where the outcome variable is the monthly Bartik shock computed from the BLS Quarterly Census of Employment and Wages (QCEW). Colored bands are ± 2 standard error bands based on robust standard errors clustered at the state level. All regressions are weighted by the number of households in 2000. The model with no controls only employs the *CA* and *HighForc* indicators as well as county and time fixed effects; the full model uses county and time fixed effects as well controls fully interacted with the time indicators.

Figure 4: Loan-Level REO Foreclosure Rate DDD Estimates



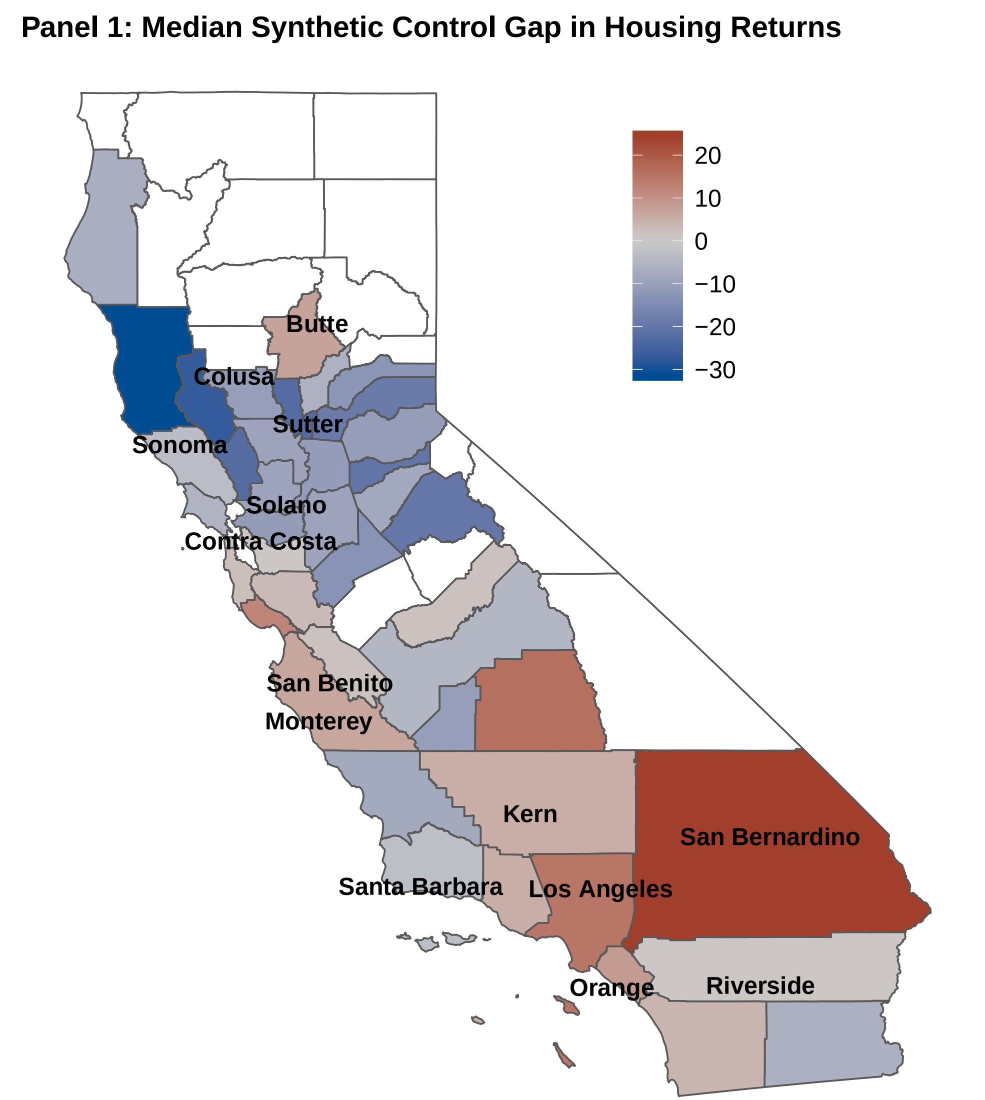
Notes: Loan-level REO foreclosure rate DDD linear probability model regressions. The left-hand-hand side variable takes a value of 1 if a loan enters REO foreclosure and zero otherwise. These regression are based on 205,558,378 loan-month observations. Estimation is implemented using a two-step procedure: First, we regress the REO foreclosure indicator variable on loan-level characteristics and zip3-month dummies and retain the coefficients on the zip3-month dummies. We allow the regression coefficients on loan-level characteristics to vary flexibly with time. Then in the second step we estimate the DDD REO foreclosure rate coefficients. The loan-level characteristics controlled for in the first step include unpaid principal balance and the interest rate origination. Loan-level controls also include a full set of dummy for the following: first time homebuyers; loan purpose; Freddie Mac; origination loan term; a mortgage insurance indicator and mortgage insurance type; occupancy status; origination channel; origination year-month; origination servicer; the loan seller; the property type; as well as ventile dummies for origination credit score, origination debt-to-income (DTI), and origination loan-to-value. Missing values for any of these variables are encoded with a separate dummy. Indeed, we use ventile dummies for variables such as DTI so that we can retain "low-documentation loans" where we employ a separate dummy variable for each variable if the value is missing (e.g. for DTI we control for 21 dummy variables: one for each ventile and an additional dummy variable for missing data). The macro controls associated with the green line include Land Unavailability as well as the QCEW and CBP Bartik shocks. The second step regression is weighted by the number of households in 2000. Colored bands are ± 2 robust standard error bars clustered at the state level.

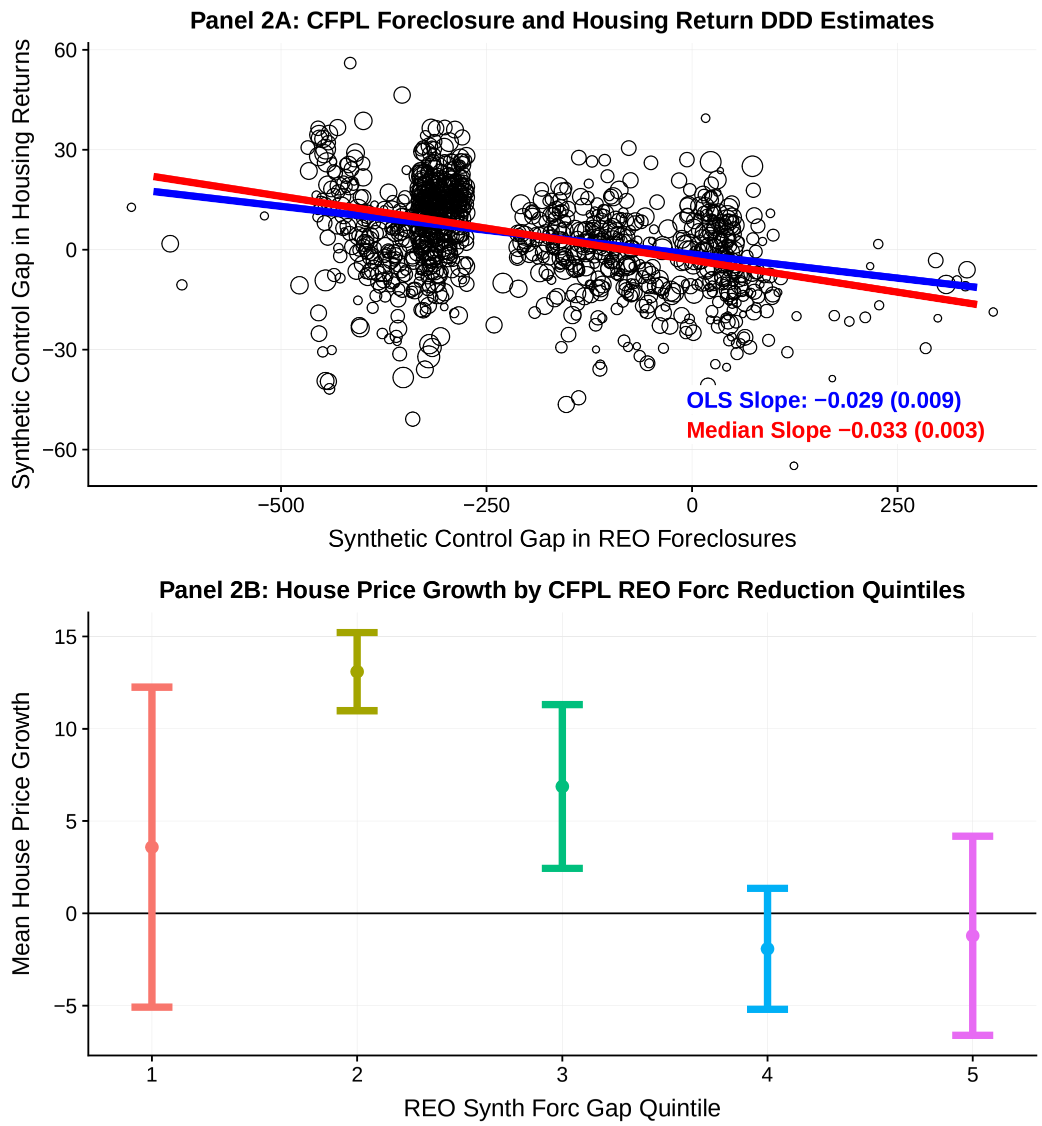
Figure 5: Loan-Level Modificaiton Rate DD Estimates



Notes: Loan-level modification rate DD linear probability model regressions. The left-hand-hand side variable takes a value of 1 if a loan enters modification and zero otherwise. These regression are based on 206,530,893 loan-month observations. For further information on model specification, see the notes to figure 4.

Figure 6: Zip Code CFPL House Price Estimates





Notes: Panel 1 shows the median zip code level Synth Gap in house price growth (%) within each California county from 2008M07-2011M12. For each California zip code, we construct a Synthetic Control using the following variables during the pre-treatment period: Housing returns; RF 2008Q3 foreclosure predictions; 2007 unemployment rate; 2007 household income; Land Unavailability; Bartik Shocks; 2005 subprime origination rate; 2005 non-owner occupied origination rate. Variables not available at the zip code level are mapped to the zip code level using the Missouri Data Bridge. The county names printed on the map correspond to those in figure 2. Panel B shows DDD OLS and Median regression estimates of the Gap in house price growth on the Gap in foreclosures. County foreclosure Gap estimates are mapped to the zip code level using the Missouri Data Bridge. Robust OLS standard errors are clustered at the 3-digit zip code level and robust quantile regression standard errors are calculated as suggested by Koenker and Hallock (2001). All regressions are weighted by the number of households in 2000. Panel 2B shows the slope estimates from separate regressions of the Gap in house price growth on the Gap in foreclosures separated by REO Synth Gap foreclosure quartiles.

Table 1: The Impact of the CFPLs on Foreclosure Maintenance and Repair Costs – Non-Judicial States

	Dependent variable:						
	Foreclosure Maintenance and Repair Costs						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CA	−57.887	169.838	187.892				
	(270.238)	(317.595)	(281.653)				
CFPL	478.728	229.776					
	(172.828)	(178.843)					
CA × CFPL	573.777	493.146	471.543	314.882	411.657	946.176	917.492
	(172.828)	(173.795)	(149.974)	(105.062)	(106.468)	(184.043)	(254.112)
Months in REO Foreclosure		314.932	324.301	412.341	411.199	420.230	423.470
		(47.288)	(44.554)	(64.068)	(60.718)	(67.045)	(68.842)
Months in REO Foreclosure^2		−3.091	−3.356	−5.222	−5.361	−5.683	−5.709
		(1.326)	(1.213)	(1.682)	(1.607)	(1.845)	(1.912)
Constant	3,016.112	1,007.346					
	(270.238)	(292.208)					
REO Forc Date FE	No	No	Yes	Yes	Yes	Yes	Yes
Zip3 FE	No	No	No	Yes	Yes	Yes	Yes
Other Loan-level Controls	No	No	No	No	Yes	Yes	Yes
Zip3 Dummies × Linear REO Forc Date Trends	No	No	No	No	No	Yes	Yes
Zip3 Dummies × Quadratic REO Forc Date Trends	No	No	No	No	No	No	Yes
Observations	31,056	31,056	31,056	31,056	31,056	31,056	31,056

Notes: Difference-in-differences regressions of the impact of the CFPLS on foreclosure maintenance and repair costs. Foreclosures are considered as in the pre-CFPL period if both the REO foreclosure date *and* the REO foreclosure disposition date are before the announcment and implementation of CFPLs in July 2008. Foreclosures are considered in the CFPL period if the REO foreclosure date is after the announcement of the CFPLs in July 2008, but before the announcement of HAMP in March 2009. Thus, these data include no loans that entered into REO foreclosure after the announcement of HAMP. The loan-level controls include a dummy variable for Freddie Mac; ventile dummies for the unpaid principal balance (origination and at foreclosure), borrower credit score, the debt-to-income ratio, the origination interest rate, and loan-to-value ratio at origination; indicator variables for occupancy status; and indicator variables for the purpose of the loan. These regressions employ data only from non-judicial states. The three-digit zip code time trends are zip code indicators multiplied by a time trend corresponding to the REO foreclosure date. Robust standard errors are clustered at the state level.

Table 2: The Impact of the CFPLs on REO Foreclosure Durations

	Dependent variable:					
	Months in REO Foreclosure (Foreclosure Duration)					
	(1)	(2)	(3)	(4)	(5)	(6)
CA	0.057			0.186		
	(0.301)			(0.208)		
CA × CFPL	−0.662	−0.573	−0.589	−0.591	−0.430	−0.475
	(0.421)	(0.313)	(0.296)	(0.329)	(0.227)	(0.215)
Avg(REO Forc Len) Non-CA, CFPL	7.970	7.970	7.970	7.773	7.773	7.773
REO Forc Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Zip3 FE	No	Yes	Yes	No	Yes	Yes
Loan-level controls	No	No	Yes	No	No	Yes
Sample	Non-judicial States	Non-judicial States	Non-judicial States	All States	All States	All States
Observations	31,652	31,652	31,652	48,673	48,673	48,673

Notes: Difference-in-differences regressions of the impact of the CFPLS on foreclosure maintenance and repair costs. See table 1 for the definition of foreclosures included in the data and the loan-level controls included. Columns (1) - (3) use only use data from non-judicial foreclosure states; columns (4) - (6) use data from all states.

Table 3: Probability of Denial and Loan Volume Growth After the CFPLs

	Dependent variable:					
	Prob(Deny)		Loan Growth ($)		Loan Growth (Num)	
	(1)	(2)	(3)	(4)	(5)	(6)
California	−0.005	−0.076	0.083	0.077	0.133	0.115
	(0.013)	(0.014)	(0.028)	(0.026)	(0.032)	(0.026)
Sample	AZ,CA, FL,NV Loan Level	CA,CO, NY,TX Loan Level	AZ,CA, FL,NV Zip Code	CA,CO, NY,TX Zip Code	AZ,CA, FL,NV Zip Code	CA,CO, NY,TX Zip Code
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Estimation Method	LPM	LPM	OLS	OLS	OLS	OLS
Observations	797,732	1,278,510	1,044	1,601	1,044	1,601

Notes: Regressions of the probability of mortgage denial and zip code level loan volume growth on an indicator for California and controls. In columns (1) and (2), the dependent variable takes a value of one if the mortgage application was denied and zero otherwise and the coefficients from a linear probability model. California takes a value of one for California and zero otherwise. Controls in columns (1) and (2) include the log of applicant income and loan amount; Zillow house price returns and IRS income and population growth in the year before the loan application was submitted; Land Unavailability; and factor variables for applicant race and applicant sex. The samples include only loans not sold to GSEs in AZ, CA, FL, and NV (column 1) and CA, CO, NY, and TX (column 2) from 2009 to 2014. Columns (3) - (4) and (5) - (6) show regressions where dollar loan volume growth or the growth in the number of loans represents the dependent variable. Loan volume growth is defined as $(\ln(\text{Loan\_vol}_{2009} + \cdots + \text{Loan\_vol}_{2014})) - (\ln(\text{Loan\_vol}_{2007}))$. The sample is restricted to loans not sold to GSEs. The key right-hand-side variable of interest is an indicator that takes a value of one for California. The data for these regressions are at the zip code level. Controls include Land Unavailability, applicant income growth and IRS income and population growth as well as Zillow zip code level house price growth for 2008-2009, 2010-2011, and 2012-2014. The regressions in columns (3) - (6) are weighted by the number of households. Robust standard errors are clustered at the 3-digit zip code level.

A Online Appendix: The California Foreclosure Prevention Laws

SB-1137:

California Senate Bill 1137 (SB-1137) was passed and implemented on July 8, 2008 and mandated that mortgage lenders operating in California delay filing an NOD until 30 days after contacting the homeowner with information on foreclosure alternatives.[18] Specifically, SB-1137 required the lender to contact the borrower in person or over the telephone and notify the borrower of his right to schedule a meeting with the lender to discuss foreclosure alternatives. The mortgagor then had the right to schedule a meeting with the lender within 14 days of first contact. Then, after the initial contact or attempted "due diligence", the law required the lender to wait 30 days before filing an NOD. Three attempts to contact the mortgagor over the telephone on different days and at different times satisfied the law's due diligence requirement. This due diligence requirement likely created large foreclosure institutional costs for lenders as many lacked the infrastructure to contact borrowers by telephone on a large scale (Agarwal et al., 2017). Further, the law required the legal owner who took possession of a vacant residential property via foreclosure to maintain it or face fines of up to $1000 per property per day.[19] The sunset date for SB-1137 was January 1, 2013.

Prior to the enactment of SB-1137, existing law only required that the lender file an NOD with the appropriate county recorder and then mail the NOD to the mortgage borrower. In sending the NOD, lenders were not obligated to provide information on foreclosure alternatives. The aim of SB-1137 was to alert struggling homeowners of foreclosure alternatives via mortgage lenders and change the net present value calculation of foreclosure versus mortgage modification.[20]

The California Foreclosure Prevention Act (CFPA):

The CFPA was signed into law on February 20, 2009 and went into effect on June 15, 2009 for the period extending through January 1, 2011. The aim of the CFPA was to provide lenders with incentives to implement comprehensive mortgage modification programs during a period of housing crisis and widespread mortgage failure. The CFPA prohibited lenders from issuing an NOS for an additional 90 days after the initial NOD *unless* the lender enacted a mortgage modification program meeting the requirements of CFPA. As a non-judicial foreclosure state, California already required a three month waiting period between the NOD and the NOS. Thus, under the CFPA, lenders that had not implemented comprehensive loan modification programs meeting the CFPA regulations were required to wait a total of six months between the NOD and the NOS.

Mortgage lenders who implemented an acceptable mortgage modification program were exempted from the additional 90 day CFPA foreclosure moratorium. To obtain this exemption, a lender's loan modification program was required to achieve affordability and sustainability targets for modified loans.[21]

To be eligible for a mortgage modification under the CFPA a borrower must (1) live in the property; (2) be in default (foreclosure); (3) document an ability to pay the modified loan; (4) have obtained the mortgage under consideration between January 1, 2003 to January 1, 2008; and (5) not have surrendered the property or engaged in a bankruptcy proceeding. The CFPA also required that mortgages under consideration for modification be the first lien on a property in California. All loans originated in California that meet the above requirements were subject to the provisions of the CFPA. Loans where a servicing or pooling agreement prohibited modification are exempt from the CFPA. The State of California also outlined a number of procedures related to the implementation of the CFPA. When a mortgage lender submitted an application for exemption under the CFPA, the State immediately issued a temporary order of exemption from the CFPA foreclosure moratorium. Then, within 30 days, the lender received a final notification of exemption or denial regarding the mortgage modification program.

An adequate CFPA modification program was required to keep borrowers in their homes when the

[18] http://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=200720080SB1137

[19] Further, SB-1137 was only applicable for mortgages on owner-occupied homes originated between January 1, 2003 and December 31, 2007.

[20] Indeed, the Bill's chaptered text cites a Freddie Mac report that suggested that 57 percent of late paying borrowers did not know that their lender may offer a foreclosure alternative.

[21] Note that lenders participating in the HAMP program were considered to be in compliance with the CFPA and thus were exempt from the extra 90 day foreclosure moratorium under the law.

anticipated recovery under the loan modification or workout exceeded the proceeds from foreclosure on a net present value basis. Mortgage modification programs were also mandated to achieve a housing-related debt to gross income ratio of 38 percent or less on an aggregate basis and contain at least two of the following features: An interest rate reduction over a fixed term for a minimum of five years; an extension of the loan amortization period up to 40 years from the original date of the loan; deferral of principal until the maturity of the loan; a reduction in principal; compliance with a federal government mortgage program; or other factors that the state Commissioner deemed appropriate. The CFPA also outlined long-term sustainability goals regarding the performance of mortgage loans modified under the CFPA. In particular, the CFPA guidelines state that a modified loan was sustainable if the borrower's monthly payment under the modified loan was reduced for five years; if the modification yielded a housing-related debt-to-income ratio of at most 38 percent; if the borrower's back-end debt-to-income ratio was no more than 55 percent (the back-end debt-to-income ratio is the total monthly debt expense divided by gross monthly income); if under the modified loan, the borrower was current on his mortgage after a three month period; or if the modification satisfied the requirements of a federal program. Applicants filing for an exemption via HAMP may have been required to submit a copy of their Servicer Participation Agreement for HAMP under the Emergency Economic Stabilization Act of 2008.

The CFPA was signed into law on February 20, 2009 and went into effect on June 15, 2009 for the period extending through January 1, 2011. In March 2009, California established a timeline for the implementation of the CFPA and posted it online; on April 21, 2009 the CA government released a draft of the emergency regulations to interested parties and accepted comments until May 6, 2009; On May 21, 2009, the emergency regulations associated with the CFPA were filed with the California Office of Administrative Law (OAL); and on June 1, 2009, the OAL approved the emergency regulations and filed them with the Secretary of State.

In total, 149 applications were submitted for exemptions from the CFPA foreclosure moratorium. Of these 149 applications, 78.5 percent were accepted, 11.5 percent were denied, and 10 percent of the applications were withdrawn. Hence, a non-trivial portion of the submitted mortgage modification programs did not meet the CFPA standards. Note also that the number of applications for the CFPA exemption was lower than anticipated as some lenders preferred the additional 90 days in foreclosure so they could avoid taking possession of non-performing properties at the height of the crisis (California, 2010).

B Online Appendix: State Synthetic Control Results

Table B1: State-Level Synthetic Control Estimation Results

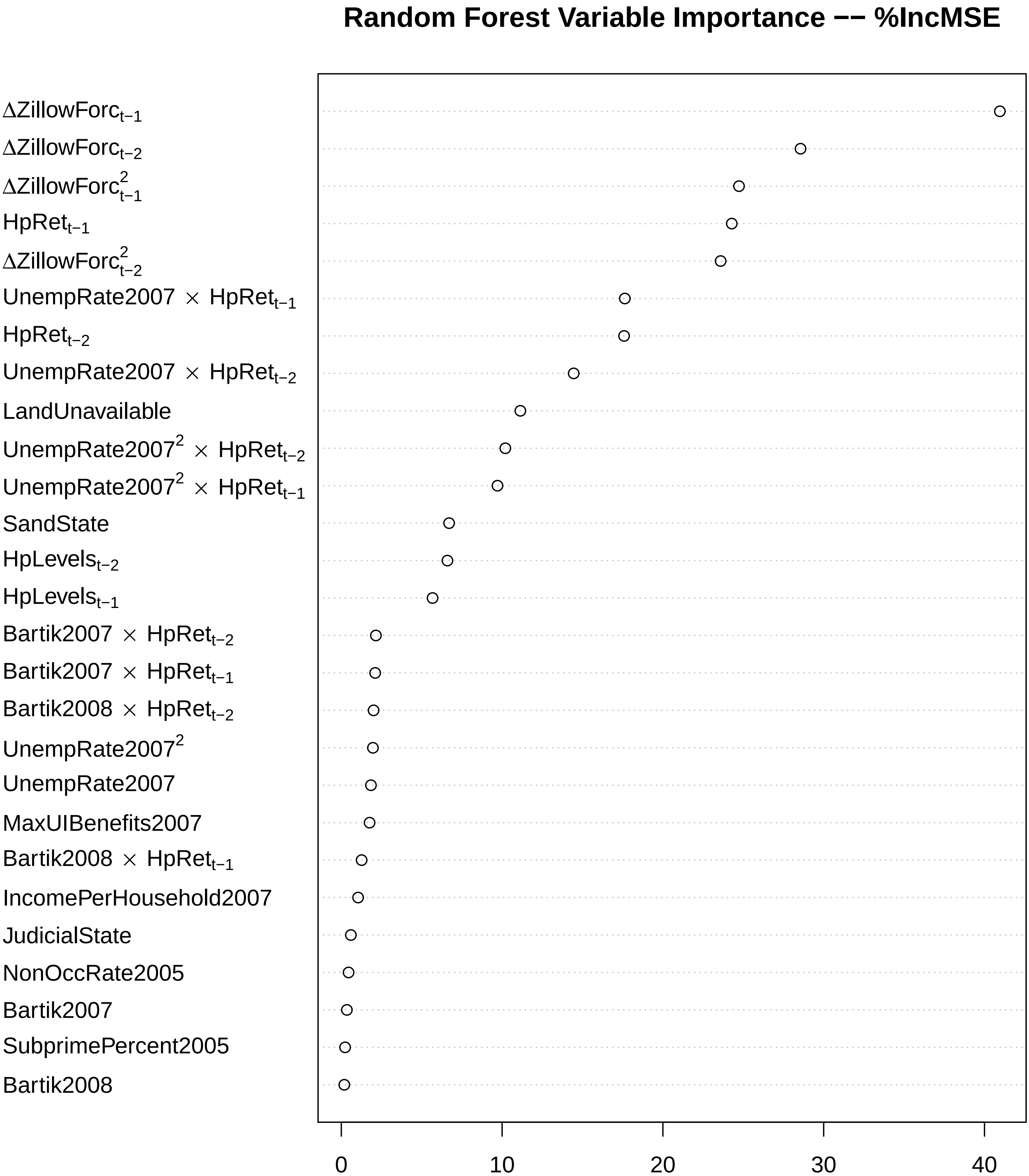
	Pre MSE (1)	CFPL Treatment Period CA (2)	Synth (3)	Gap (4)	Gap Pcntle (5)
Panel A: Foreclosures and the MDRI					
Forc Starts (% of All Loans)	0.01	20.90	32.81	-11.91	0.00
Prime Forc Starts (% of Prime Loans)	0.00	17.26	29.08	-11.82	0.00
Subprime Forc Starts (% of Subprime Loans)	0.03	59.18	83.40	-24.22	0.00
Zillow REO Forc per 10,000 Homes	0.95	839.71	1208.86	-369.15	0.00
Mortgage Default Risk (MDRI)	0.01	25.47	53.52	-28.04	0.00
Panel B: House Price Growth					
FHFA HP Growth (%)	0.40	-25.25	-47.94	22.69	100.00
Zillow All Homes HP Growth (%)	0.03	-28.48	-46.01	17.53	100.00
Zillow Bottom Tier HP Growth (%)	0.05	-47.67	-70.98	23.31	100.00
Zillow Top Tier HP Growth (%)	0.04	-13.56	-23.34	9.78	91.18

Notes: The far left column lists the outcome variable, Pre-MSE in column (1) is the mean-squared error from the Synthetic control match during the pre-treatment period (2004M01-2008M06), the next two columns (2 & 3) show the change in the outcome variable for California and its Synthetic Control during the CFPL treatment period, and Gap in column (4) is the difference between of the change in the outcome variable for treated unit (California) relative to its Synthetic Control. The MDRI over the treatment period is the cumulative sum in its index points over the treatment period. Column (5) shows the percentile of the Gap estimate relative to all placebo effects. This is calculated by first estimating the empirical CDF from all placebo effects and then calculating the percentile of the Gap for California relative to the CDF of placebo effects. The treatment period ranges from 2008M07 to 2011M12. The variable descriptions and data sources are in the notes to figure 1 and in appendix C.

C Online Appendix: Data List

Variable	Source
State Level Data	
Foreclosure Starts (Notice of Default); (% of All Loans)	Mortgage Bankers' Assoc
Prime Forc Starts (% of Prime Loans)	Mortgage Bankers' Assoc
Subprime Forc Starts (% of Subprime Loans)	Mortgage Bankers' Assoc
Zillow REO Foreclosures per 10,000 Homes	Zillow
Mortgage Default Risk (MDRI)	Chauvet et al. (2016)
FHFA All Transaction House Price Returns	FHFA
Zillow All Homes Returns	Zillow
Zillow Bottom Tier Returns	Zillow
Zillow Top Tier Returns	Zillow
County Level Data	
Zillow REO Foreclosures per 10,000 Homes	Zillow
Zillow All Homes Returns	Zillow
Unemployment Rate	BLS
Land Unavailability	Lutz and Sand (2017)
Bartik Labor Demand Shocks	Compiled From County Business Patterns
Bartik Labor Demand Shocks	Compiled From BLS QCEW
Maximum Unemployment Benefits	Hsu et al. (2018) (State)
Income Per Household, 2007	IRS Statistics of Income
% of Subprime Mortgage Loans, 2005	HMDA & HUD Subprime List
Non-Occupied Occupation Rate (NonOccRate), 2005	HMDA & Gao et al. (2017)
Zip Code Level Data	
Zillow All Homes Returns	Zillow
Unemployment Rate	BLS (county level)
Land Unavailability	Lutz and Sand (2017)
Bartik Labor Demand Shock	Compiled From CBP (County)
Maximum Unemployment Benefits	Hsu et al. (2018) (State)
Income Per Household, 2007	IRS Statistics of Income
% of Subprime Mortgage Loans, 2005	HMDA & HUD Subprime List
Non-Occupied Occupation Rate (NonOccRate), 2005	HMDA & Gao et al. (2017)
Loan Level Data	
GSE Loan Performance	Fannie Mae & Freddie Mac
Home Mortgage Disclosure Act (HMDA)	FFIEC

D Online Appendix: Random Forest ΔForeclosure Variable Importance



Notes: Variable importance for each variable, %IncMSE, is the percentage increase in the MSE from that variable being randomly shuffled and re-computing the Random Forest predictions. Larger numbers indicate a higher MSE after the given variable is randomly shuffled, indicating higher variable importance.

E Online Appendix: Loan-level Foreclosure Alternate Rate DDD Estimates

Figure 7: Loan-Level Foreclosure Alternate Rate DDD Estimates

Notes: Loan-level foreclosure alternate rate DDD linear probability model regressions. The left-hand-hand side variable takes a value of 1 if a loan becomes a foreclosure alternate (Short Sale, Third Party Sale, Charge Off, Note Sale) and zero otherwise. For further information on model specification, see the notes to figure 4.

F Online Appendix: CFPLS and Maintenance Costs – All States

Table A1: The Impact of the CFPLs on Foreclosure Maintenance and Repair Costs – All States

	Dependent variable:						
	Foreclosure Maintenance and Repair Costs						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CA	−56.529	105.762	154.533				
	(160.125)	(193.103)	(172.979)				
CFPL	518.554	227.229					
	(115.278)	(115.337)					
CA × CFPL	533.950	475.247	423.616	262.886	338.503	919.915	838.276
	(115.278)	(112.701)	(100.301)	(81.246)	(85.976)	(135.388)	(188.824)
Months in REO Foreclosure		328.373	334.543	412.282	407.824	413.435	416.606
		(41.607)	(41.094)	(44.527)	(41.949)	(45.949)	(46.996)
Months in REO Foreclosure^2		−2.967	−3.145	−4.909	−4.927	−5.105	−5.129
		(1.083)	(1.045)	(1.230)	(1.186)	(1.337)	(1.387)
Constant	3,014.754	984.459					
	(160.125)	(196.514)					
REO Forc Date FE	No	No	Yes	Yes	Yes	Yes	Yes
Zip3 FE	No	No	No	Yes	Yes	Yes	Yes
Other Loan-level Controls	No	No	No	No	Yes	Yes	Yes
Zip3 Dummies × Linear REO Forc Date Trends	No	No	No	No	No	Yes	Yes
Zip3 Dummies × Quadratic REO Forc Date Trends	No	No	No	No	No	No	Yes
Observations	47,887	47,887	47,887	47,887	47,887	47,887	47,887

Notes: See the notes for table 1. This table uses data from all states.

G Online Appendix: CFPL Zip Code House Price Growth Estimates

Table G1: Zip Code CFPL DDD Regressions – Foreclosures and House Price Growth

	Dep Var: Synth Gap in Housing Returns			
	OLS		Median Reg	
	(1)	(2)	(3)	(4)
Synthetic Control Gap in Foreclosures	−0.029	−0.028	−0.033	−0.038
	(0.009)	(0.009)	(0.003)	(0.004)
Bartik Shock 2009 - 2011		−0.318		1.205
		(1.535)		(0.737)
Household Income in 2007 ($000s)		−0.068		−0.061
		(0.021)		(0.018)
House Price ($000s)		0.021		0.017
		(0.005)		(0.003)
Constant	−1.349	−9.402	−0.794	2.417
	(1.715)	(11.904)	(0.739)	(5.825)
Observations	1,079	1,079	1,079	1,079
R^2	0.101	0.162		

Notes: Regressions are weighted by the number of households in 2000. For OLS, robust standard errors are clustered at the three digit zip code level. For quantile median regression estimates, standard errors are computed using a robust Huber sandwich esimtate as suggested by Koenker and Hallock (2001).