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

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We investigate the impact of Great Recession policies in California that substantially increased lender pecuniary and time costs of foreclosure. We estimate that the California Foreclosure Prevention Laws (CFPLs) prevented 250,000 California foreclosures (a 20% reduction) and created $300 billion in housing wealth. The CFPLs boosted mortgage modifications and reduced borrower transitions into default. They also mitigated foreclosure externalities via increased maintenance spending on homes that entered foreclosure. The CFPLs had minimal adverse side effects on the availability of mortgage credit for new borrowers. Altogether, findings suggest that policy interventions that keep borrowers in their homes may be broadly beneficial during times of widespread housing distress. (*JEL* E20, E65, H70, R20, R30)

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At the height of the 2000s housing boom, California accounted for one-quarter of U.S. housing wealth.[1] But as the 2006 boom turned into the 2008 bust, house

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[1] ACS Table-S1101 and Zillow.

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prices in the state fell 30%, and over 800,000 homes entered foreclosure.[2] To aid distressed borrowers, stem the rising tide of foreclosures, especially in the hard-hit areas of Southern California and the Inland Empire, and combat the crisis, the State of California in 2008 enacted unique foreclosure abatement and forbearance legislation (the California Foreclosure Prevention Laws). The new laws increased foreclosure pecuniary costs to mitigate maintenance-related foreclosure externalities, while simultaneously imposing delays and foreclosure moratoria on lenders to encourage mortgage modification. Unlike later federal programs, the California policy treatment effects were broad-based and immediate.[3] Yet despite the application of a unique policy to the nation's largest 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).

In California, lenders can foreclose on deeds of trust or mortgages using a nonjudicial foreclosure process (outside of court).[4] Prior to the CFPLs, the state required only that a lender or servicer (henceforth, lenders) initiating a home foreclosure deliver a notice of default (foreclosure start) to the borrower by mail. A 90-day waiting period then commenced before the lender could issue a notice of sale 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).[5] This bill, which immediately went into effect, mandated that agents who obtained a vacant residential property through foreclosure must maintain the property or face steep fines of up to $1,000 per property per day. SB-1137 also prohibited lenders from issuing a notice of default to owner-occupied borrowers 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. These foreclosure mediation statutes also applied to borrowers who were issued a notice of default prior to July 2008 but were awaiting a notice of sale, meaning that SB-1137 aimed to dampen both foreclosure starts and real estate–owned (REO) foreclosures (when a buyer loses their home to the

[2] Mortgage Bankers Association.

[3] Major federal programs that were implemented with a large delay following announcement included the Home Affordable Modification Program (HAMP) and the Home Affordable Refinance Program (HARP). See Agarwal, Amromin, Chomsisengphet, et al. (2015) and Agarwal, Amromin, Ben-David, et al. (2017) for an overview of these programs.

[4] For an overview of the judicial foreclosure process and its impacts, see Pence (2006); Ghent and Kudlyak (2011); Gerardi, Lambie-Hanson, and Willen (2013); Mian, Sufi, and Trebbi (2015). California is one of several U.S. states known as nonjudicial foreclosure states. Other states require foreclosures to be processed via the local courts and hence are known as judicial foreclosure states.

[5] California Senate Bill 1137, Residential mortgage loans: foreclosure procedures, available at http://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=200720080SB1137

financial institution) upon passage. The following year, in June 2009, California implemented the California Foreclosure Prevention Act (CFPA). The CFPA imposed an additional 90-day moratorium after the notice of default on lender conveyance to borrowers of a notice of sale unless the lender implemented a state-approved mortgage modification program. Together, the CFPLs (SB-1137 and the California Foreclosure Prevention Act) significantly increased the lender pecuniary and time costs of home foreclosure. A full overview of the CFPLs is in Online Appendix A.

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 by encouraging foreclosure maintenance spending and mortgage modification.

From the outset, the CFPLs were viewed with skepticism. In marked contrast to the California approach, the U.S. 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.[6]

However, recent academic studies suggest mechanisms whereby the CFPLs could have bolstered California housing markets. The key economic channel is based on the negative price impacts of foreclosure on the foreclosed home and neighboring properties, whereby foreclosures adversely affect nearby housing by increasing housing supply, or through a “disamenity” effect where distressed homeowners neglect home maintenance.[7] 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, losses in both employment and house prices lead to further foreclosures (Foote, Gerardi, and Willen 2008; Mian, Sufi, and Trebbi 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.

6 Summers’s and Geithner’s comments were related to increasing foreclosure durations. Neither Summers nor Geithner mentioned policies that incentivized maintenance spending on foreclosed homes. Timothy Geithner, interview by Charlie Rose, October 13, 2010, https://www.youtube.com/watch?v=sXxnGbOp5cU. Lawrence Summers, “Lawrence Summers on ‘House of Debt,”’ *Financial Times*, June 6, 2014, https://www.ft.com/content/3ec604c0-ec96-11e3-8963-00144feabdc0.

7 For the foreclosure impacts on housing supply, see Campbell, Giglio, and Pathak (2011); Anenberg and Kung (2014); Hartley (2014). Studies that examine the disamenity effects of foreclosures include Harding, Rosenblatt, and Yao (2009); Gerardi et al. (2015); Lambie-Hanson (2015); Cordell and Lambie-Hanson (2016); Glaeser, Kincaid, and Naik (2018). Also see Morse and Tsoutsoura (2013); Munroe and Wilse-Samson (2013); Gupta (2019); Biswas et al. (2019).

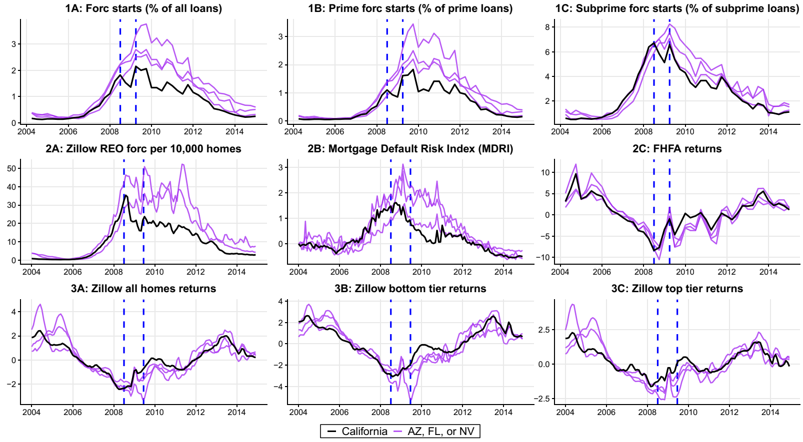

Figure 1
Sand State foreclosures, mortgage distress, and housing returns
Plots of foreclosures, mortgage distress, and housing returns for Arizona, California, Florida, and Nevada. The black line is California, and the purple lines represent Arizona, Florida, or Nevada. The first dashed-blue vertical line signifies the passage of 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, Gabriel, and Lutz (2016); and housing returns are from the FHFA and Zillow. See the data list in Online Appendix C for more information on data sources.

Figure 1 presents motivating evidence regarding the impacts of the CFPLs via plots of housing indicators for California and the other Sand States (Arizona, Florida, and Nevada; in the literature, the Sand States are typically grouped together as they experienced a similar housing market boom and bust and collectively were the epicenter of the late-2000s housing crisis). The blue-dashed vertical lines represent the inception dates of SB-1137 and the California Foreclosure Prevention Act. First, all Sand States behaved similarly prior to the CFPLs (for example, the parallel pre-trends difference-in-differences assumption), and there were no levels differences between California and the other Sand States during the pre-CFPL period. Then, with the passage of the CFPLs, California foreclosures and mortgage default risk fell markedly and housing returns increased; these effects persisted through the end of the sample in 2014. In a preview of our main results, we apply the synthetic control method to these indicators in Table B1 and Figure B1 of Online Appendix B, where the potential cross-sectional controls consist of all U.S. states. The results show that following the implementation of the CFPLs, the improvement in the California housing market was large in magnitude compared with the estimated counterfactual. Further, falsification tests in which we iteratively apply the treatment to all other states (a permutation test), shown in Table B1 (Column 5; see Table notes for computational details), indicate that the estimated response to treatment in California housing markets was rare, akin to statistical significance in traditional inference.

The key identifying assumption in the aforementioned synthetic control analysis and throughout our study is that we can generate a counterfactual that would represent the path of California housing markets in the absence of the treatment. The threats to such an identification strategy are (i) differential California macro trends that may contaminate comparisons of treatment and controls; and (ii) confounding outsized local employment or house price shocks unrelated to the treatment in California housing markets, relative to controls, that may reduce foreclosures in California (noting from the double trigger theory of mortgage default (Foote, Gerardi, and Willen 2008) that households default on mortgages when faced with the interaction of negative equity and an adverse employment shock).

To establish internal validity of our CFPL estimates and address potential confounds, we exploit the sharp nature of the CFPL policy experiment, disaggregated data, within-California and across-state variation, and several estimation approaches to account for local housing and macro dynamics, loan-level characteristics, and California-specific macro trends in our identification of policy effects. Specifically, in support of a causal interpretation of our results, we note the following: (i) The implementation of the CFPLs resulted in an immediate change in California housing markets upon announcement, well before federal programs, making other explanations for our results unlikely;[8] (ii) our results are robust across multiple identification schemes that account for California macro trends and anomalous shocks to non-California housing markets by exploiting the state-level nature of the policy, border analyses, and only within-California variation; (iii) findings are consistent across both loan-level and aggregated data compiled from different sources; (iv) our results are robust to the inclusion of multiple housing, employment, and loan-level controls; (v) we implement multiple falsification tests to examine the CFPLs relative to other housing markets or economic variables where the results are congruent with a causal interpretation of the CFPL effects; and (vi) we document the direct CFPL impacts for the targeted owner-occupied homes, relative to non-owner-occupied homes, on foreclosure starts looking only within California zip codes as well as on foreclosure maintenance spending and modifications.

In total, our findings suggest that the CFPLs were highly effective in stemming the crisis in California foreclosures. The CFPLs prevented 250,000 REO (notice of sale) foreclosures, a reduction of 20%, and increased California aggregate housing returns by 5%. In doing so, they created \$300 billion of housing wealth. These effects were concentrated in areas most severely hit by the crisis. Indeed, in the local California housing markets in which

[8] Federal programs such as the Home Affordable Modification Program (HAMP) and the Home Affordable Refinance Program (HARP).

CFPL foreclosure reduction was most pronounced, house prices increased on average by more than 10% relative to counterfactuals. We further provide direct evidence that the CFPLs positively affected housing markets using loan-level micro data: in a within-zip-code, California-only difference-in-differences research design, we find that SB-1137 reduced foreclosure starts (notice of defaults) for the targeted owner-occupied borrowers, relative to the non-owner-occupied borrowers that were not subject to SB-1137's notice of default delay. Moreover, our results show 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 policy incentives (recall that SB-1137 mandated that agents who took over foreclosed properties must maintain them or face fines of up to $1,000 per day). This increased maintenance and repair spending directly mitigates the foreclosure "disamenity" effect, a key reason why foreclosures create negative externalities.[9] As SB-1137 increased the cost of REO foreclosure via increased maintenance and repair spending, and as longer REO foreclosure durations (for example, the time from when the lender takes possession of 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 foreclosure durations. This is a key policy goal of a foreclosure mediation strategy and matches what we find in our analysis of the policy, congruent with the CFPLs increasing foreclosure costs.[10] In other direct evidence of CFPL impacts, we also show that the CFPLs increased mortgage modifications. Specifically, we find that before the implementation of the federal government's main housing programs that the CFPLs increased the mortgage modification rate by 38%.[11] Finally, we find that the policies did not create any adverse side effects for new California borrowers as regards credit rationing. This result is congruent with expectations given the prominence of the government-sponsored enterprises (GSEs) in mortgage lending following the Great Recession and as the GSEs do not discriminate based on geography (Hurst et al. 2016).

In sum, our results suggest that the CFPLs were a successful global financial crisis-era intervention that substantially reduced mortgage default, decreased home foreclosure, and boosted house prices. While the CFPLs were implemented at the height of the Great Recession in some of the nation's hardest hit housing markets, policymakers have pursued similar interventions during other crises. These other policy interventions provide further experimental opportunities to assess the external validity of our CFPL results. For example,

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[9] See Gerardi et al. (2015); Lambie-Hanson (2015); Cordell and Lambie-Hanson (2016); Glaeser, Kincaid, and Naik (2018).

[10] Timothy Geithner, interview by Charlie Rose, *Charlie Rose*, October 13, 2010, https://www.youtube.com/watch?v=sXxnGbOp5cU.

[11] These federal housing programs included the Home Affordable Modification Program (HAMP) and the Home Affordable Refinance Program (HARP).

Rucker and Alston (1987) document that foreclosure moratoria reduced farm foreclosures during the Great Depression. Likewise, in response to the recent COVID-19 pandemic, the United States passed the CARES Act to allow COVID-19 affected mortgage borrowers to enter mortgage forbearance and thus delay their mortgage payments. Like the CFPLs, the aim of COVID-19 induced CARES Act mortgage forbearance was to keep borrowers in their homes during a period of widespread housing and financial market distress. We thus view the study of mortgage forbearance during the COVID-19 crisis as both a promising avenue for future research and as a potential opportunity to test the external validity of the CFPL policy response.

1. 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; the portion of subprime loans originated from Home Mortgage Disclosure Act (HMDA) data and the Housing and Urban Development (HUD) subprime originator list; and the non-occupied homeowner occupation rate, as this may be a predictor of house price growth (Gao, Sockin, and Xiong 2020). We discuss these data in context in this section and list all data in Online Appendix C.

We also assess the effects of the CFPLs using loan-level data from the Fannie Mae and Freddie Mac (GSEs) loan performance data sets. We use GSE loan performance data for two key reasons: First, the GSE data are publicly available, making our analysis transparent and reproducible. Second, and just as important, the GSEs apply similar lending standards across regions and do not discriminate based on geography (Hurst et al. 2016), meaning that the set of GSE loans yields natural control and treatment groups as regards the support of loan-level characteristics.[12] Moreover, we supplement this data with the Moody's Blackbox data set that covers the universe of data sold into private-label mortgage-backed securities. We discuss our identification strategy for our loan-level analysis in depth in the following section.

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[12] Note that the GSE data contain a large number of subprime loans originated during the 2000s housing boom. Following the literature and defining subprime loans as loans where the borrower has a credit score below 660, between 2004 and 2006 during the height of the boom, 1.29 million originated loans in California in the GSE data set were subprime representing 15.3% of all originations. Likewise, for the U.S. overall during this period, 15.5% of originated loans in the GSE data set were subprime. The similar subprime origination rates in California and the United States overall also highlight how the GSEs apply a consistent lending methodology across geographies and that GSE mortgages thus constitute a natural control and treatment group in our analysis.

2. Estimation Methodology: CFPLs and County REO Foreclosures

We employ two main separate estimation schemes to measure the effects of the CFPLs on foreclosures at the county level: The synthetic control method (Abadie, Diamond, and Hainmueller 2010; Abadie, Diamond, and Hainmueller 2015) and a difference-in-difference-in-differences approach. Our other analyses (for example, loan-level estimates) build on the approach described here.

2.1 Synthetic control

The synthetic control (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, the synthetic control approach 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 the pretreatment period. For our foreclosure analysis, we iteratively construct a synthetic control unit for each California county. The characteristics used to build the synthetic units are discussed in Section 3. The CFPL policy effect is the difference (gap estimate) between each California county and its synthetic control.

A key advantage of the synthetic control approach is that it uses pretreatment characteristics to construct the a weighted average of the control group from all potential candidates. The synthetic control method therefore nests the usual difference-in-differences research design, while extending this approach to remove researcher choice and ambiguity as regards the construction of the control group. Hence, as suggested by Athey and Imbens (2017), synthetic control provides a simple, yet clear improvement over typical methods and is arguably the most important innovation in policy evaluation since 2000.[13]

Using the synthetic control framework, we also generate localized policy estimates for each California county. This allows us to assess the distribution of policy estimates across the geography of California as well as ensure that average overall estimates are not generated by particular a county or local housing market.

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 (Acemoglu et al. 2016). For California counties where gap estimates extend beyond these confidence intervals, the CFPL effects are rare and large in magnitude, akin to statistical significance in traditional inference.

[13] See Athey and Imbens (2017) and the references therein for broad overview of the synthetic control literature and how it compares to other methods.

2.2 Difference-in-difference-in-differences (triple-differences):

We also estimate the foreclosure impacts of the CFPLs through a triple-differences research design that exploits a predictive framework that measures ex ante expected variation in REO foreclosures both within California and across other states. Generally, the triple-differences 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 triple-differences specification for foreclosures is as follows:

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

The dependent variable is Zillow REO foreclosures per 10,000 homes. *CA* and *HighForc* are indicators for California and high-foreclosure counties, respectively. We define *HighForc* based on pretreatment attributes as discussed later. The excluded dummy for indicator and static variables is 2008M06, the month prior to the first CFPL announcement. The coefficients of interest, the triple-differences estimates, 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 toward the end of the sample.

Intuitively, for each month y, θ_y is the difference-in-difference-in-differences in foreclosures where we compare 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 triple-differences 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 triple-differences policy estimate over the whole CFPL period is $\Theta = \sum_{y \geq 2008M07} \theta_y$, 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 in the following section) 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 triple-differences approach by mimicking Equation 1 with the synthetic control estimates and regressing the synthetic control gaps on *HighForc* interacted with month indicators using only the California data in the final regression. This approach follows from the observation that the synthetic control 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 data set, we retain county and time fixed effects but interact the controls only with a CFPL indicator.

To measure the county-level pre-CFPL expected exposure to foreclosures (*HighForc*), we use only pre-CFPL data to forecast the increase (first-difference) in foreclosures (Δforeclosures) in each county for 2008Q3, the first CFPL treatment quarter, using only data up to 2008Q2 (pretreatment data). A random forest model is used to build the forecasts, as random forest models often provide more accurate predictions than traditional techniques (Breiman 2001; Mullainathan and Spiess 2017; Athey 2018) and as the random forest approach implements automatic variable selection (Breiman 2001). Thus, the strength of the random forest for our setup is that it allows us to include the large array of foreclosure predictors previously identified in the literature and let the data and model decide which variables are most important, removing ambiguous choice as regards predictor inclusion. Furthermore, by automatically combining these predictors to reduce forecast error variance, the random forest model is likely to yield more accurate foreclosure predictions than traditional techniques such as ordinary least squares (OLS).

We first train the random forest model using data available up to 2008Q1; this first step uses all pre-CFPL data. We then move one step ahead and predict Δforeclosures out-of-sample for 2008Q3, the first CFPL treatment quarter, using data up to 2008Q2. Predictors used in our random forest 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, Gerardi, and Willen 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, Sufi, and Trebbi 2015); the 2005 non-owner-occupied mortgage origination rate as a proxy of housing market speculation (Gao, Sockin, and Xiong 2020); and the maximum unemployment benefits for each county's state in 2007 (Hsu, Matsa, and Melzer 2018). Predictors also include 2007 income per household, a Sand State indicator, and pre-CFPL Bartik (1991) labor demand shocks.[14] We also interact the Bartik shocks with housing returns. Variable importance for each predictor in the random forest model is plotted in Online Appendix D.

To gauge predictive accuracy, we evaluate our random forest predictions relative to traditional OLS models using the mean-squared error (MSE) for non-California counties in 2008Q3. The mean-squared error for the random forest model is 36.5% lower relative to a benchmark panel AR(2), indicating that the random forest predictions are substantially more accurate. The mean-squared error of the random forest model is also 60.1% lower than a full OLS model that includes all aforementioned predictors.[15]

We classify counties as either high or low foreclosure (*HighForc*) based on the random forest predictions using a cross-validation approach. Specifically, we search from the U.S. median predicted change in foreclosures for 2008Q3 (1.64 per 10,000 homes) to the 90th percentile (13.07 per 10,000 homes) and choose the cutoff for high-foreclosure counties that minimizes the pretreatment difference between the treatment and control groups in Equation 1 (the cutoff that minimizes $\sum_{y<2008M07}\theta_y^2$). The cutoff chosen by the cross-validation procedure is 7.54 REO foreclosures per 10,000 homes, corresponding to the 82nd percentile, meaning that *HighForc* counties have a predicted increase in foreclosures of at least 7.54 per 10,000 homes for 2008Q3.

Note also that the random forest model predicts marked foreclosure increases for the mean low-foreclosure California county at 5.28 REO foreclosures per 10,000 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 triple-differences estimates to account for California macro-level trends that may lower foreclosures across the state.

The controls for the triple-differences model in Equation 1 include the Quarterly Census of Employment and Wages (QCEW; monthly) and County Business Patterns (CBP; annual) Bartik labor demand 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.

[14] For a recent analysis of Bartik performance, see Albouy et al. (2019).

[15] We also compare the performance of the random forest model to a autoregressive panel model with only lags of foreclosures and house price returns, as these are the top two predictors in the random forest model. We find in our out-of-sample test that the MSE for the random forest model is 29% lower than the MSE for this autoregressive panel model. Thus, the other variables and the random forest model yield predictive power beyond just a linear inclusion of lags of foreclosures and house price returns.

3. The Impact of the CFPLs on Foreclosures

3.1 County-level REO foreclosure analysis

The estimates of the CFPL impacts on REO foreclosures using the synthetic control and triple-differences approaches are visualized in Figure 2. The county-level attributes used to build the synthetic matches for each California county use only pretreatment data and include the following: random forest predictions for Δforeclosures in 2008Q3, REO foreclosures, the 2007 county-level unemployment rate, land unavailability, the Bartik shock between 2007M03 and 2008M03, the percentage of subprime originations in 2005, the non-owner-occupied origination rate in 2005, Zillow house price growth in the first six months of 2008, and the interaction of the unemployment rate in 2007 and house price growth of the first six months of 2008 in line with a double trigger for mortgage default.

Panel 1A plots the cumulative gap in real estate owned (REO) foreclosures at various percentiles for California counties, where the percentiles are calculated within each month using only the California county-level synthetic control 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

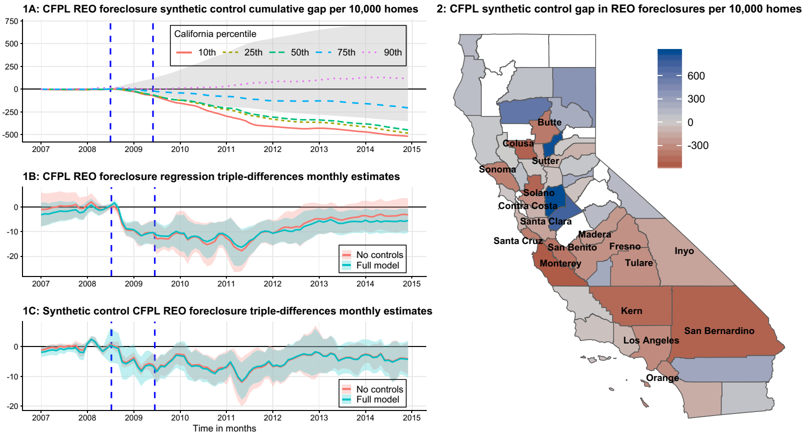

Figure 2
CFPL REO foreclosure estimates
Panel 1A shows the synthetic control cumulative gap in county-level REO foreclosures per 10,000 homes for California counties grouped by percentile. The two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA in 2008M07 and 2009M06, respectively. The gray band represents a 95% 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 10,000 homes from 2008M07 to 2011M12 across California counties. Counties in white have no data. County names are printed on the map if their gap in REO foreclosures per 10,000 homes is in the bottom 5th percentile relative to the empirical CDF of all estimated placebo effects. Panel 1B shows the monthly 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. Panel 1C is the implementation of Equation 1 using the synthetic control output where ± 2 error bands correspond to robust standard errors clustered at the county level.

policy effect. Gap estimates that jut outside this confidence band are rare and large in magnitude, corresponding to statistical significance in traditional inference.

During the pretreatment period, the cumulative gap is near zero across California percentiles, in line with the parallel pre-trends assumption. Online Appendix E shows the top counterfactual regions for California counties; overall, the results match our expectations where pretreatment high-foreclosure California regions are matched to high-foreclosure regions in other states.[16] Then, with the passage of SB-1137 in 2008M07, foreclosures drop immediately for California counties at the 50th, 25th, and 10th percentiles. Counties at these percentiles are also bunched together toward 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 foreclosures. Hence, the CFPL effects were not driven by a sole county or local housing market. The decline in foreclosures for these counties continued through 2014, consistent with long-lasting policy effects and contrary to concerns expressed by federal policymakers, as 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 synthetic control cumulative gap in REO foreclosures from 2008M07 to 2011M12. Red areas represent a reduction in foreclosures relative to the synthetic 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 cumulative distribution function (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, a lower-income and supply elastic region in California's Inland Empire, was the epitome of the 2000s subprime crisis. This county subsequently experienced large and beneficial CFPL policy effects: REO foreclosures per 10,000 homes in San Bernardino fell by 525.33 (28.2%). Relative to the synthetic control counterfactuals, foreclosure reductions were also large in Los Angeles and

[16] For inland Southern California regions, such as San Bernardino County, the synthetic control approach places a large weight on areas in the other Sand States, like those in Nevada and Arizona. In marked contrast, for the highest income counties in the Bay Area like San Francisco County, the synthetic control algorithm draws the control group largely from New York County (where Manhattan is located), King County (Seattle) other counties in Maryland, and other areas that were not hit hard by the housing crisis. The benefit of the synthetic control approach is that it uses extensive data to select control units appropriate to each treated unit, so that the researcher does not have to make those decisions based on limited information (Athey and Imbens 2017).

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 synthetic control estimates across all California counties, results imply that the CFPLs prevented 250,000 REO foreclosures, a reduction of 20.2%.[17]

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 standard errors are clustered at the state level to account for autocorrelation and spatial correlation across local housing and labor markets within each state.

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 pretreatment period, the ± 2 standard error bands subsume the horizontal origin, and thus the parallel pre-trends assumption is satisfied. Third, and congruent with the foregoing synthetic control 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.[18] 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 rollout 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 triple-differences 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 10,000 homes. This estimate is in line with our synthetic control results.

Last, panel 1C of Figure 2 mimics Equation 1 and panel 1B, but uses the synthetic control output and only within-California data as discussed earlier 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

[17] Reestimating our synthetic control results using only nonjudicial states in the control group suggests that the CFPLs reduced foreclosures by 20.8%.

[18] Agarwal, Amromin, Chomsisengphet, et al. (2015) and Agarwal, Amromin, Ben-David, et al. (2017).

implementation of SB-1137; remains below the zero axis and thus documents a reduction of foreclosures due to the CFPLs until 2012; and then returns to zero at the end of the sample period, implying no reversal in policy effects.

In Online Appendix F we consider several robustness tests and falsification tests and also examine only within-California variation. First, we find that our triple-differences estimates are robust to the inclusion of county linear and quadratic time trends. This test supports the parallel pre-trends assumption and implies that the CFPLs induced a sharp and immediate reduction in California foreclosures. Next, Online Appendix F explores a number of additional controls and falsification tests based on the theoretical drivers of foreclosures from the double trigger theory of mortgage default (Foote, Gerardi, and Willen 2008): house price growth, employment shocks, and their interaction. Overall, the results suggest that our CFPL findings are robust to these controls and that there were no outsized employment shocks coinciding with the announcement and implementation of the CFPLs. Last, we consider only within-California variation; these results are congruent with our main findings.

3.2 CFPL difference-in-difference-in-differences REO foreclosure loan-level estimates

One potential concern with our analysis is that loan-level characteristics may differ across regions and thus contaminate our 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 (i) they are publicly available; and (ii) the GSEs do not discriminate across regions, yielding loans that constitute natural control and treatment groups within a difference-in-difference-in-differences (triple-differences) analysis. Our outcome of interest is the probability that a mortgage enters REO foreclosure, and we aim to estimate the triple-differences coefficients via a linear probability model that emulates Equation 1. We retain data from only nonjudicial foreclosure states, as these represent a natural control group for California during the Great Recession. Overall, as shown here, our results after accounting for loan-level characteristics match the findings that employ county-level, aggregated data.

We proceed with estimation by employing a common two-step reweighting technique (Borjas 1987; Altonji and Card 1991; Card 2001).[19] This approach allows us to recover the underlying micro, loan-level triple-differences estimates after controlling for loan-level characteristics, while accounting for the fact that REO foreclosure and loan disposition are absorbing states (for example, once a loan enters REO foreclosure or is refinanced, it is removed from the data set) and thus that the number of loans available in each region during each time period may in itself depend on the treatment.

[19] For more recent references, see Angrist and Pischke (2008); Beaudry, Green, and Sand (2012); Lutz, Rzeznik, and Sand (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 incorporates three-digit zip codes (zip3):

$$\text{Prob(REO Forc)}_{it} = \sum_{y=1}^{T} \sum_{j=\text{zip3}(1)}^{\text{zip3}(N)} (\rho_{jy} \times \mathbf{1}\{y=t\} \times \text{zip3}_{ij}) + \sum_{y=1}^{T} (\mathbf{1}\{y=t\} \times \mathbf{X}_i' \boldsymbol{\tau}_y) + e_{it} \tag{2}$$

The dependent variable for loan observation i at year-month t is an indicator that takes a value of one for REO foreclosure and zero otherwise. ρ_{jy} are the zip3-month coefficients on zip3 $\times \mathbf{1}\{y=t\}$ dummy variables, and τ_y 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 2 allows us to quality-adjust and thus purge our estimates from any bias associated with differences in loan-level characteristics. We estimate Equation 2 using only loans originated during the pretreatment 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 measured only at loan origination, as time-varying variables (such as current unpaid principal balance) may also be affected by program treatment. $\mathbf{X}_i$ includes a wide array of loan characteristics that are listed in the notes to Figure 3, which shows our final estimation output.

From the regression in Equation 2, we retain the zip3-month coefficient estimates on the zip3 $\times$ $\mathbf{1}\{y=t\}$ dummy variables, ρ_{jy}. In the second step of the estimation process, we employ the following model, which yields the triple-differences estimates of the impact of the CFPLs on the probability of REO foreclosure at the loan level (slightly changing the subscripts on ρ to match Equation 1):

$$\begin{aligned} \rho_{it} = & \sum_{\substack{y=1 \\ y\neq 2008\text{M}06}}^{T} (\theta_y \mathbf{1}\{y=t\} \times HighForc_i \times CA_i) \\ & + \sum_{\substack{y=1 \\ y\neq 2008\text{M}06}}^{T} (\mathbf{1}\{y=t\} \times (\beta_{1y} HighForc_i + \beta_{2y} CA_i + \mathbf{X}_i' \boldsymbol{\lambda}_y)) \\ & + \mathbf{X}_{it}' \boldsymbol{\gamma} + \delta_t + \delta_i + \varepsilon_{it} \end{aligned} \tag{3}$$

θ_y is the triple-differences 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

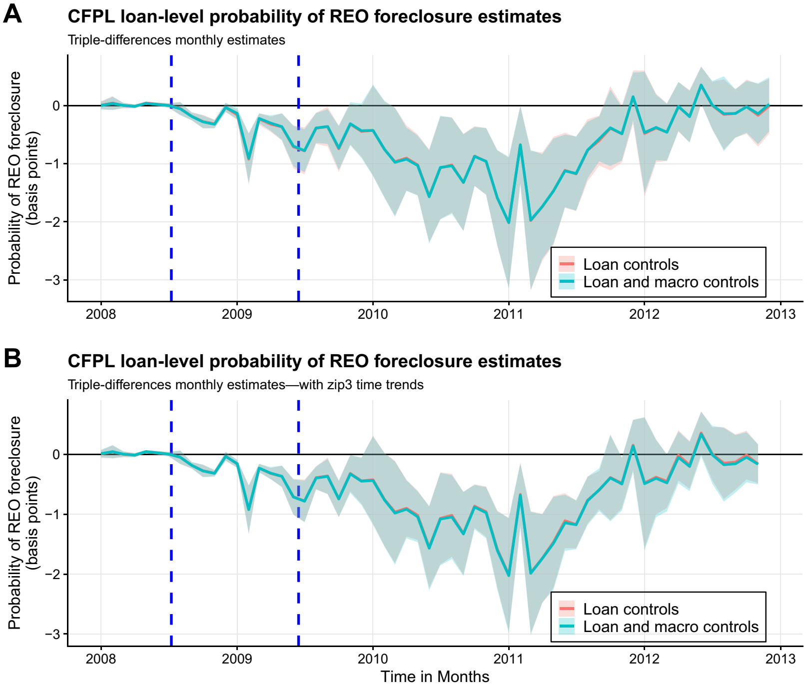

Figure 3
Loan-level REO foreclosure rate triple-differences estimates
Loan-level REO foreclosure rate triple-differences linear probability model regressions. The left-hand side variable takes a value of one if a loan enters REO foreclosure and zero otherwise. These regressions are based on 205,558,378 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 triple-differences REO foreclosure rate coefficients. The loan-level characteristics controlled for in the first step include unpaid principal balance and the interest rate at origination. Loan-level controls also include a full set of dummy variables 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.

high- and low-foreclosure zip3 regions in other states. We determine high-foreclosure California zip3 regions based on the random forest predictions and the process documented earlier. Aggregate controls include land unavailability as well as Census County Business Patterns (CBP) and BLS Quarterly Census of Employment and Wages (QCEW) Bartik labor demand shocks.

The results are in Figure 3. The second-step regression in Equation 3 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 particular loan is quite small.

The path of θ_y in panel A, Figure 3 (both with and without extra macro and housing controls), matches our previous triple-differences estimates in Figures 2 and F1, implying that our estimates of the impact of the CPFLs on REO foreclosures are robust to the inclusion of loan-level characteristics as controls.

First, during the pretreatment period, θ_y is a precisely estimated zero, indicating that the parallel pre-trends assumption is satisfied. 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 REO foreclosure rate for high-foreclosure California regions, relative to a counterfactual of non-California high-foreclosure regions, fell by 38% due to the CFPLs. The cluster-robust F-statistic associated with the triple-differences estimate during the pre-HAMP treatment period $(\sum_{y=2008M07}^{2009M02}\theta_y)$ is 21.0 (p-value <0.001), meaning that the reduction in REO foreclosures following 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 results that show the CFPLs simply did not delay REO foreclosures until a later date.

Panel B of Figure 3 controls for zip3 time trends and therefore assesses the parallel pre-trends assumption and whether 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 3. 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 being disposed via a foreclosure alternative (short sale, third party sale, charge off, or note sale). While foreclosure alternatives may reduce the number of empty homes, such resolutions would not have aided policymakers in their goal of keeping homeowners in their homes. We repeat our analysis, but let the dependent variable be equal to one for mortgages that enter into a foreclosure alternate and zero otherwise. The path of the triple-differences coefficients is in Online Appendix G. The results show that there was no change in the incidence of foreclosure alternates during the early part of the crisis. 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.

3.3 CFPL transition probabilities from default to foreclosure

Next, we examine the transition rates of distressed mortgages into foreclosure. The research question is whether distressed California mortgages were less likely to enter foreclosure due to the CFPLs, as distressed loans were the primary target of the policy (later, we also evaluate cure rates for mortgages in default). We measure delinquency in the month prior to the CFPL announcement, June 2008, so that the CFPLs do not contaminate the measured initial delinquency status. We then trace out transition probabilities from pre-CFPL delinquency to foreclosure. As mortgages sold into private securitization constituted an outsized number of defaults, for this analysis we use the universe of private-label mortgages from Moody's BlackBox. This allows us to employ a within-delinquency cohort analysis that yields similarity between treatment and control mortgages in terms of distress and assesses the robustness of our foregoing results to private-label securitized mortgages. Likewise, we consider loans from Arizona, California, and Nevada to ensure comparability of housing and default conditions across treatment and control groups. Note that a drawback of this research design is that the CFPL treatment can affect delinquency status. Thus, we can only measure delinquency status in the pre-CFPL period and examine the subsequent transition probabilities for these loans, whereas our earlier analysis allowed us to consider all loans.

We first examine the transition probabilities into REO foreclosure of loans that were 90 days delinquent in the month before the CFPLs, noting that 90-day delinquencies typically correspond to borrower default and an initiation of the foreclosure process. Our key generalized difference-in-differences estimating equation becomes

$$\begin{aligned}
&\text{Prob}(\text{REO Foreclosure}_{it}|\text{Default}_{\text{Pre-CFPL}})\\
&\quad= \sum_{\substack{y=1\\ y\neq 2008\text{M}06}}^{T} (\theta_y \mathbf{1}\{y=t\} \times CA_i)\\
&\qquad+ \sum_{\substack{y=1\\ y\neq 2008\text{M}06}}^{T} (\mathbf{1}\{y=t\} \times \mathbf{X}_i' \boldsymbol{\lambda}_y) + \delta_t + \text{zip3}_i + \varepsilon_{it} \qquad (4)
\end{aligned}$$

where θ_y is the difference-in-differences estimate that measures the probability of transition from default in June 2008, the month before the CFPL announcement, to REO foreclosure for mortgages in California relative to those in control states. For estimation, we employ the two-step procedure discussed earlier. $\mathbf{X}_i$ is a large array of loan characteristics measured at origination, and we

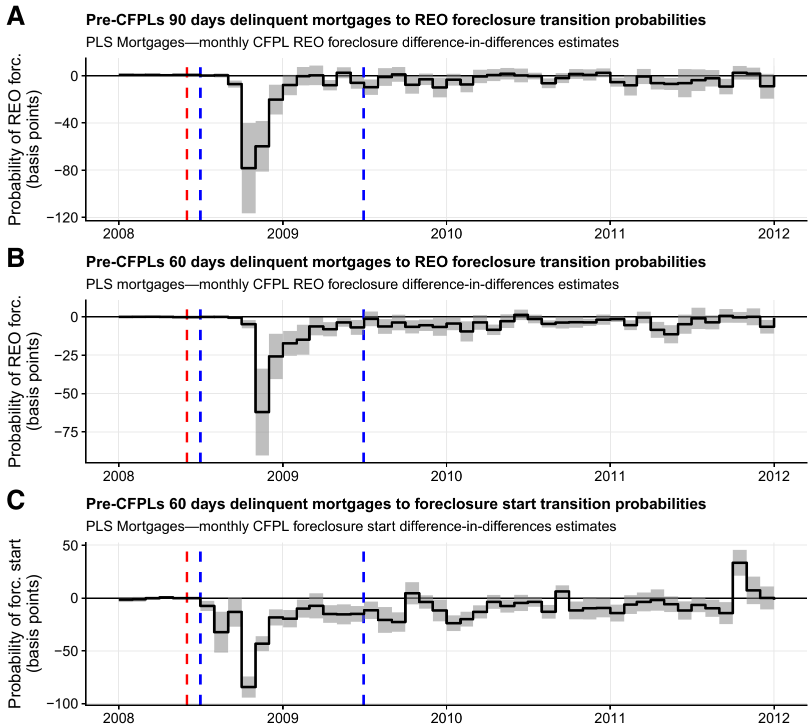

Figure 4
Transition probabilities from default to foreclosure
Loan-level year-month Moody's BlackBox private-label mortgage loans sold into private-label securitization (PLS). The red-dashed vertical line represents when delinquency status was measured, the month before the CFPL announcement in June 2008. The two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA, respectively. Loan-level controls include three-digit zip code and time fixed effects; dummy variables for the origination year-month; indicator variables for contract loan type including whether or not the loan is a hybrid ARM, an option ARM, or a negative amortization mortgage; if it had a balloon payment, an interest-only period, and an ARM loan that could be converted into a fixed rate loan; the origination balance; the FICO credit score and LTV at origination; dummy variables for the interest rate index for ARM loans with a separate variable for fixed rate loans; and fixed effects for the following variables: loan purpose, property type, and servicer. Data are from Arizona, California, and Nevada. Gray bands correspond to ±2 robust standard errors clustered at the three-digit zip code level.

allow the coefficients on these controls to vary flexibly with time. The full list of controls are in the notes to Figure 4. Panel A of Figure 4 displays the estimates of θ_y from Equation 4. The red-dashed vertical line is the month prior to the CFPL announcement (June 2008), when delinquency status was measured, and the blue lines are the implementations of SB-1137 and the CFPA, respectively. Gray bands correspond to ±2 robust standard errors clustered at the three-digit zip code level.

As expected, there is no difference in the probability of REO foreclosure between treatment and control mortgages prior to the CFPLs as REO foreclosure is an absorbing state. Hence, the parallel pre-trends assumption is satisfied by construction. Similarly, there is no change in the probability of foreclosure for the first three months after delinquency measurement due to the requisite duration between foreclosure initiation and REO foreclosure. Then, in late 2008, distressed California mortgages that were 90 days delinquent in June 2008 experienced a sizable and statistically significant drop in the probability of foreclosure. Note that our estimates here are markedly larger than comparable foregoing estimates for all GSE mortgages. This implies the CFPL foreclosure impacts were strongest for the most at-risk California borrowers, in line with the policy targeting distressed households. Then, into 2009 and through the end of the sample there were some slight decreases in the transition rate into REO foreclosure and no evidence of reversal. Hence the CFPL policy effects were long-lasting.

Next, panels B and C show the difference-in-differences estimates for the transition rates of loans 60 days delinquent in the month prior to the implementation of the CFPLs to REO foreclosure (panel B) and foreclosure starts (panel C), building on the regression model in Equation 4. Starting with panel B, where the dependent variable is (Prob(REO Foreclosure)$_{it}$|60 Days Delinquent$_{\text{Pre-CFPL}}$), we document a large decline in the probability of transition from 60 days delinquent to REO foreclosure.[20] The initial decline is smaller than the estimated reduction for 90-day delinquent loans in panel A but longer lasting. Also congruent with panel A there is no evidence of reversal in the CFPL effects, indicating that the initial CFPL foreclosure reduction for 60-day delinquent loans did not reverse in later periods.

Finally, in panel C, where the dependent variable is (Prob(Foreclosure Start)$_{it}$|60 Days Delinquent$_{\text{Pre-CFPL}}$), results show that the CFPLs led to a decline in the transition probability from 60-day delinquency to a foreclosure start. These effects lasted through 2010, and there is no substantial evidence of reversal toward the end of the sample period.

Overall, panels A and B document a marked CFPL reduction in the transition to REO foreclosure for seriously delinquent loans, while panel C suggests that the CFPLs impeded foreclosure starts. Together, this evidence further supports CFPL efficacy, as the policies increased foreclosure costs to lower the transition of delinquent loans into foreclosure.

3.4 Alternative identification: CFPL border analysis

In the previous analysis, we employed all regions within in California to measure the total impact of the CFPLs on foreclosures. As an alternative form of identification, we also conduct a border analysis using California, Arizona, and Nevada. An important benefit of a border analysis research design in our context is that the California eastern border region is largely separated and

20 Note the decline in the probability of transition from 60-day delinquency to REO foreclosure occurs later for these loans compared with those in panel A, as the lower delinquency status necessitates a longer transition duration to REO foreclosure.

dissimilar from California's large coastal population centers likely targeted by the CFPLs. Thus, the CFPL policy shock may be more plausibly exogenous for these regions. A second advantage of the border analysis is that the regions on either side of the border are more likely to be similar in terms of economic and population dynamics. Yet the notable drawback of any border design is that the estimates from this analysis may have limited external validity when applied outside of the border region.

We estimate two versions of the border analysis using the three-digit zip codes adjacent to the California border. First we consider only the Lake Tahoe border community along the Northern California eastern border with Nevada. While this community is smaller than California's larger cities and not large enough to be an MSA, it extends across the California and Nevada border. We construct a map of the three-digit zip codes used in this analysis in Online Appendix Figure H1. In a second approach, we use loans from all three digit zip codes along the Arizona, California, and Nevada borders. We plot these regions in Online Appendix Figure H2. As there are a limited number of three-digit zip codes and we intend to geographically cluster standard errors, we employ the Moody's BlackBox data that covers the universe of mortgages sold into private securitization and report the zip code for each mortgage. Furthermore, as the CFPL policy has an implementation date, we can exploit the time dimension of policy, which is not available for other border foreclosure studies such as Mian, Sufi, and Trebbi (2015). We thus employ a loan-level difference-in-differences analysis across the California border and over time. The estimating equation builds on our previous analyses as follows:

$$\text{Prob(REO Foreclosure}_{it}) = \sum_{\substack{y=1 \\ y \neq 2008\text{M06}}}^{T} (\theta_y \mathbf{1}\{y=t\} \times CA_i) + \mathbf{X}_i' \boldsymbol{\lambda} + \delta_t + \text{zip}_i + \varepsilon_{it} \tag{5}$$

We estimate Equation 5 for both the Lake Tahoe region and for the full Arizona, California, and Nevada border regions. As the zip codes near the border regions are geographically large, we control for zip code rather than the three-digit zip codes used earlier. Finally, robust standard errors are clustered at the four-digit zip code level and the loan-level controls are listed in the notes to Figure 5, which displays our final estimation output.

The estimation results for θ_y are plotted in Figure 5, where panel A shows the output from the Lake Tahoe region and panel B displays the output from the full border analysis. The results indicate that the CFPLs lowered REO foreclosures for homes on the California side of the border. Unfortunately, due to the small number of observations, the standard errors for the Lake Tahoe border region in panel A are quite wide. Nevertheless, the results indicate that there was a large and statistically significant reduction in REO foreclosures for Lake Tahoe homes on the California side of the border. The difference-in-differences

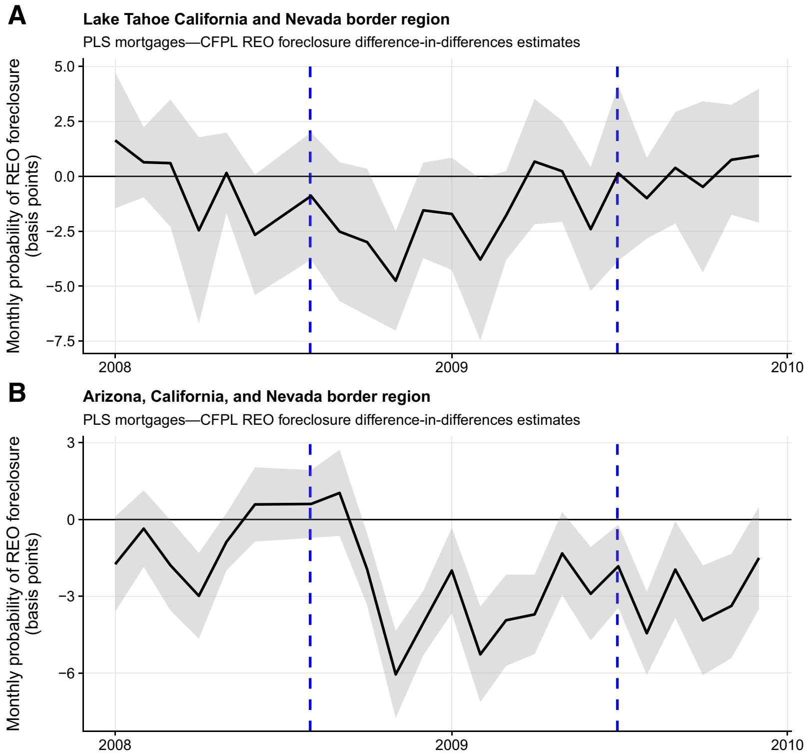

Figure 5
CFPL border difference-in-differences analysis
Loan-level year-month Moody's BlackBox private-label mortgage border analysis using private-label mortgage loans sold into private-label securitization (PLS). The two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA, respectively. Loan-level controls include zip code and time fixed effects; dummy variables for the origination year-month; indicator variables for contract loan type including whether or not the loan is a hybrid ARM, an option ARM, or a negative amortization mortgage; if it had a balloon payment, an interest-only period, and is an ARM loan that could be converted into a fixed rate loan; the origination balance; the FICO credit score and LTV at origination; dummy variables for the interest rate index for ARM loans with a separate variable for fixed rate loans; and fixed effects for the following variables: owner-occupied status, loan purpose, property type, and servicer. As in the panel for the Lake Tahoe region, there are a limited set of observations; the gray bands are ±2 standard errors. In the bottom panel the gray bands are ±2.5 standard errors. Standard errors are clustered at the four-digit zip code level. The border regions used in panels A and B are mapped in Online Appendix Figures H1 and H2, respectively.

estimates are much more precise when we consider the full border region in panel B, corresponding to the large increase in observations. Here, congruent with our previous findings, the implementation of the CFPLs leads to a sizable and immediate reduction in foreclosures and no subsequent reversal in policy effects. Overall, our border analysis results thus further support efficacious CFPL policy effects within an important research design comprising a high likelihood of internal validity.

3.5 Direct evidence of CFPL foreclosure impacts

In this section, we provide direct evidence of the CFPL effects by first using only California mortgages to show that the policies lowered initial defaults (foreclosure starts) and increased modifications for the targeted owner-occupied homes, relative to non-owner-occupied homes. We then examine foreclosure maintenance and repair costs for homes in REO foreclosure (once the mortgage borrower had been evicted) along with REO foreclosure durations and find the CFPLs increased foreclosure maintenance spending and decreased REO foreclosure durations, thus limiting the negative externalities of foreclosure. It is important to note that the CFPLs increased foreclosure costs with the aim of keeping borrowers in their homes and encouraging modification along multiple dimensions, including: (i) mandating that lenders contact borrowers regarding foreclosure alternatives before initiating the foreclosure process; (ii) fining the agents who did not maintain vacant residential properties obtained during foreclosure; and (iii) imposing foreclosure moratoria on lenders without adequate mortgage modification programs. With regard to the increase in foreclosure maintenance spending documented in the following section, we note that lenders have two main options in lieu of paying maintenance-related fines. Lenders could allow borrowers to stay in their homes or sell the vacant home more quickly. The potential size of the fines and uncertainty over the duration of REO foreclosure at the height of the crisis could have outsized impacts on distressed lenders facing multiple foreclosures. Indeed, lenders would choose the profit-maximizing (or lowest cost) option pertinent to the house in question. Earlier, we documented an immediate reduction in REO foreclosures after SB-1137, congruent with lenders avoiding fines by allowing borrowers to remain in their homes. Likewise, we show in the next section that the CFPLs reduced foreclosure durations, in line with lenders circumventing fines by reducing vacant home holding periods. Moreover, we also note that the increased direct costs and uncertainty created by the policies, especially at the height of the financial crisis, as well as the strong impact of foreclosure externalities, implies that the direct effects that we document here can, in combination, have an outsized impact on California foreclosure reduction.[21]

3.5.1 Owner-occupied versus non-owner-occupied homes within California. We begin by using only California mortgages to compare default probabilities for owner-occupied and non-owner-occupied homes. Recall that the foreclosure moratoria imposed by the CFPLs was limited to, and thus directly targeted, owner-occupied homes. In particular for owner-occupied homes, SB-1137 prohibited lenders from issuing a notice of default until 30 days

[21] See Harding, Rosenblatt, and Yao (2009); Campbell, Giglio, and Pathak (2011); Morse and Tsoutsoura (2013); Munroe and Wilse-Samson (2013); Anenberg and Kung (2014); Hartley (2014); Gerardi et al. (2015); Lambie-Hanson (2015); Cordell and Lambie-Hanson (2016); Glaeser, Kincaid, and Naik (2018); Gupta (2019).

after informing the borrower of foreclosure alternatives. The targeting of owner-occupied properties was consistent with long-standing U.S. social policy goals seeking to preserve and enhance the homeownership attainment of the typical American household; further, research from Freddie Mac showed that owner-occupied borrowers were unaware of foreclosure alternatives available from their lender.[22] This provision did not apply to non-owner-occupied investment properties. Hence, we use only California mortgages and a within-zip code difference-in-differences analysis to gauge the impacts of the CFPLs on default by exploiting the owner-occupied dimension of the policy. A sizable number of loans associated with non-owner-occupied homes were sold into private securitization, and we intend to conduct our analysis by comparing homes within each zip code. We thus employ the Moody's BlackBox data that comprise the universe of homes sold into private securitization.

Building on our previous analyses, the difference-in-differences equation is

$$\text{Prob(Foreclosure Start)}_{it} = \sum_{\substack{y=1 \\ y \neq 2008\text{M}06}}^{T} (\theta_y \mathbf{1}\{y=t\} \times \mathit{OwnerOccupied}_i) \tag{6}$$

$$+\mathbf{X}_i'\boldsymbol{\lambda} + \delta_t + \text{zip}_i + \varepsilon_{it}$$

where θ_y signifies the difference-in-differences estimate in the probability of a foreclosure start for owner-occupied homes relative to non-owner-occupied homes in month y relative to 2008M06.[23] Note here that we are comparing owner-occupied and non-owner-occupied homes within each zip code by controlling for zip code fixed effects. Also, we allow the coefficients in the loan-level controls ($\mathbf{X}_i$) to vary flexibly with time. Loan-level controls are listed in the notes to Figure 6.

Figure 6, panel A, shows the results where the gray bands correspond to ± 2 robust standard errors clustered at the three-digit zip code level. First note that there is no pre-CFPL difference in the probability of a foreclosure start for owner-occupied versus non-owner-occupied homes, and thus the parallel pre-trends assumption is satisfied. Then, with the implementation of SB-1137 in July 2008, there is a large and statistically significant drop in the probability that owner-occupied homes, relative to non-owner-occupied homes, enter foreclosure. These effects then persist through the end of 2009. We are cautious and do not report results after 2009, as the CFPLs may have induced general equilibrium effects via foreclosure externalities and thus contaminate long-run estimates when comparing owner-occupied and non-owner-occupied homes within each California zip code. Nonetheless, the foreclosure start reduction

[22] The bill's chaptered text cites a Freddie Mac report that suggested that 57% of late-paying borrowers did not know that their lender may offer a foreclosure alternative.

[23] We do not consider REO foreclosures here as the foreclosure maintenance fines applied to all REO foreclosures regardless of initial occupancy status.

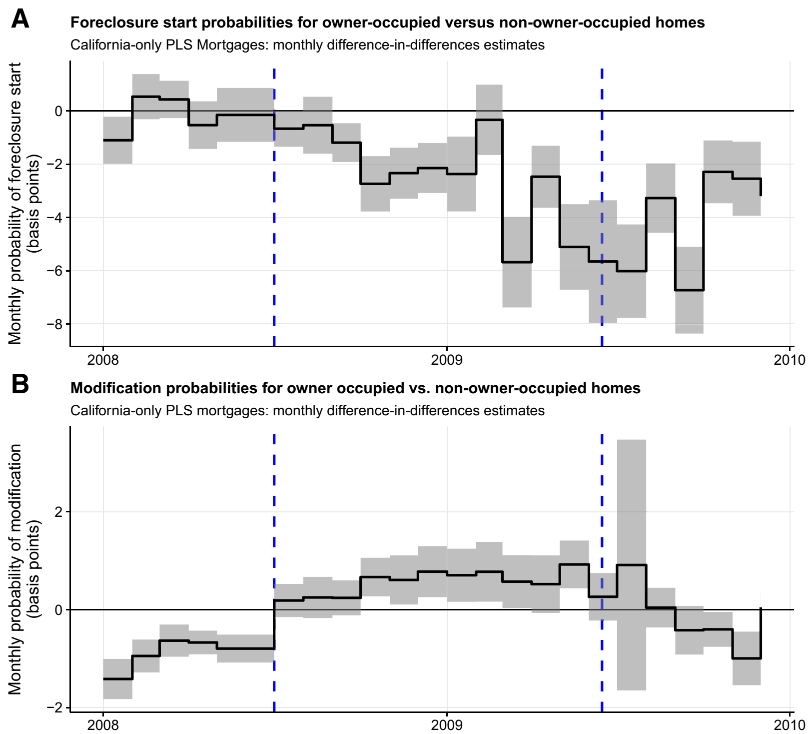

Figure 6
Owner-occupied versus non-owner-occupied homes in California
Loan-level year-month Moody's BlackBox private-label owner occupied analysis for only California mortgages using private-label mortgage loans sold into private-label securitization (PLS). The two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA, respectively. Loan-level controls include zip code and time fixed effects; dummy variables for the origination year-month; indicator variables for contract loan type including whether or not the loan is a hybrid ARM, an option ARM, or a negative amortization mortgage; if it had a balloon payment, has an interest-only period, and is an ARM loan that could be converted into a fixed rate loan; the origination balance; the FICO credit score and LTV at origination; dummy variables for the interest rate index for ARM loans with a separate variable for fixed rate loans; and fixed effects for the following variables: loan purpose, property type, and servicer. Data are from California only. The gray bands correspond to ±2 standard errors clustered at the three-digit zip code level.

effects extend through the end of the sample and do not reverse, meaning that the CFPLs reduced and did not simply delay foreclosure starts for owner-occupied homes.

Next, we consider mortgage modifications for owner-occupied versus non-owner-occupied homes within each California zip code. Here we also reimplement Equation 6, but let the dependent variable be the probability of mortgage modification. The results displayed in Figure 6, panel B, are noteworthy. First, during the pre-CPFL period, the modification rate was statistically lower for owner-occupied homes. As the excluded dummy is June 2008, this result may reflect anticipation effects where lenders began implementing their modification programs just prior to the CFPL implementation date. Then with the announcement and implementation of SB-1137 in July 2008, the modification rate for owner-occupied homes spiked and became statistically larger relative to non-owner-occupied homes in late 2008 and into 2009.

Together, panels A and B of Figure 6 using a within-zip code, California-only analysis provide evidence that the directly targeted owner-occupied homes experienced lower foreclosure starts and higher mortgage modification rates, matching the intended policy effects.

3.5.2 Foreclosure maintenance and repair spending. In this section we consider foreclosure maintenance and repair costs for homes in REO foreclosure, where an increase in these costs would represent a direct CFPL policy effect. Recall that a key provision of SB-1137 was that agents who took over a home via REO foreclosure were required to maintain the home or face fines of to \$1,000 per property per day. This implies that policymakers believed that (i) homes in REO foreclosure were not being properly maintained and (ii) that foreclosure neighborhood externality "disamenity effects" were exacerbating the foreclosure crisis. Indeed, as noted in the introduction, previous research shows that neighborhood "disamenity effects" are a key contributor to foreclosure externalities. By limiting disamenity effects via required home maintenance, the CFPLs could help stabilize home values and hence reduce foreclosures within a housing market. Further, policy-led increases in foreclosure costs change the net-present-value calculation of foreclosure relative to modification.

From the GSE loan performance data, we retain all loans that enter into REO foreclosure. For each REO foreclosure, the GSEs report the amount spent on maintenance and repairs for each home prior to disposition. The pretreatment and CFPL treatment groups are based on the REO foreclosure date. For the pretreatment 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.[24] With this data in hand, we estimate a difference-in-differences regression where the dependent variable is foreclosure maintenance and repair costs:

$$\text{Forc Maintenance and Repair Spending}_{it} = \alpha + \text{zip3}_i + \delta_t + \theta(\text{CA}_i \times \text{CFPL}_t) + \mathbf{X}_i'\boldsymbol{\lambda} + \varepsilon_{it} \tag{7}$$

where the left-hand-side variable measures foreclosure maintenance and repair spending in dollars, δ_t represents REO foreclosure date fixed effects, and the

[24] 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.

Table 1
The impact of the CFPLs on foreclosure maintenance and repair spending – nonjudicial states

	Dependent variable:						
	Foreclosure Maintenance and Repair Spending ($'s)						
	(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)	(254.111)
Months in REO Foreclosure		314.932	324.301	412.341	411.199	420.230	423.470
		(47.288)	(44.554)	(64.068)	(60.718)	(67.044)	(68.841)
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 nonjudicial 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.

coefficient of interest, the difference-in-differences estimate θ, captures the increase in foreclosure maintenance spending due to SB-1137. Note that given our definition of the treatment and control groups (based on REO foreclosure date and disposition date), 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 effects in the months spent in REO foreclosure as well as REO foreclosure date fixed effects.

The results for nonjudicial states are in Table 1, those for all states are in Online Appendix I. Column (1) of Table 1 shows the results without any fixed effects or controls. Average foreclosure maintenance and repair spending for non-California properties during the pre-CFPL period was $3,016.11. The coefficient on CA is near zero at $-57.89 dollars with a standard error of $270.24, implying that there were no average level differences in pretreatment 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 during the CFPL period for non-California foreclosures, 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 difference-in-differences estimate, is \$573.78 and statistically significant. This coefficient estimate suggests that on average 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 average monthly spending falls as durations lengthen. This may be 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 pretreatment 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 changes only slightly. Comparing average foreclosure maintenance spending after accounting for foreclosure durations suggests that the 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 at \$411.66 in Column (5) with a full set of controls and is statistically 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 difference-in-differences coefficients will be precisely estimated only if there is a sharp increase in foreclosure spending following the introduction of SB-1137. In Columns (6) and (7), the difference-in-differences coefficient is again large in magnitude and highly significant, thus implying that even after allowing for uncommon trends there was a large and statistically significant increase in foreclosure maintenance and repair spending for California properties.

3.5.3 REO foreclosure durations. The previous section documents that the CFPLs induced agents who took over homes via REO foreclosure to increase maintenance and repair spending. If the extra maintenance spending comprised marginal costs associated with length of time in foreclosure (for example, lawn maintenance), we would expect rational agents on the margin to circumvent these costs by disposing of homes obtained through REO foreclosure quicker.

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	Nonjudicial states	Nonjudicial states	Nonjudicial 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 nonjudicial foreclosure states; Columns (4)–(6) use data from all states.

In other words, REO foreclosure durations would 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.[25]

Using a difference-in-differences analysis, we assess the impact of the CFPL REO foreclosure duration effects in Table 2. Foreclosures are split into the pretreatment and treatment groups as in Section 3.5.2.[26] Columns (1)–(3) show the results for nonjudicial states only, while Columns (4)–(6) display the regression output where the data set 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 the average REO foreclosure duration for non-California properties in nonjudicial 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 were no levels differences in average REO foreclosure durations during the pretreatment period and thus that the parallel pre-trends assumption is satisfied. The coefficient on the CA × CFPL interaction is −0.662, and thus foreclosure durations fell by over half a month for California properties. Yet, as this coefficient is

25 Timothy Geithner, interview by Charlie Rose, October 13, 2010, https://www.youtube.com/watch?v=sXxnGbOp5cU.

26 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.

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 implies 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 difference-in-differences 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 difference-in-differences 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), which includes all controls, is -0.475 with a standard error of 0.215. Congruent with our previous results, this statistically significant difference-in-differences estimate means that the CFPLs shortened foreclosure durations by just under a half a month during the CPFL period.

4. Mortgage Modifications

While the overarching aim of the CFPLs was to reduce foreclosures, the policy also sought to increase modifications. This section first 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 in Section 3.2, 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 3.2, but we use an indicator for mortgage modification as the left-hand-side variable. In the second step, we estimate the following difference-in-differences regression:

$$\rho_{it} = \sum_{\substack{y=1 \\ y \neq 2008M06}}^{T} (\mathbf{1}\{y=t\} \times (\theta_y CA_i + \mathbf{X}_i' \boldsymbol{\lambda}_y)) + \mathbf{X}_{it}' \boldsymbol{\gamma} + \delta_t + \delta_i + \varepsilon_{it} \tag{8}$$

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, which 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 Census County Business Patterns (CBP) and BLS Quarterly Census of Employment and Wages (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 difference-in-differences 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.

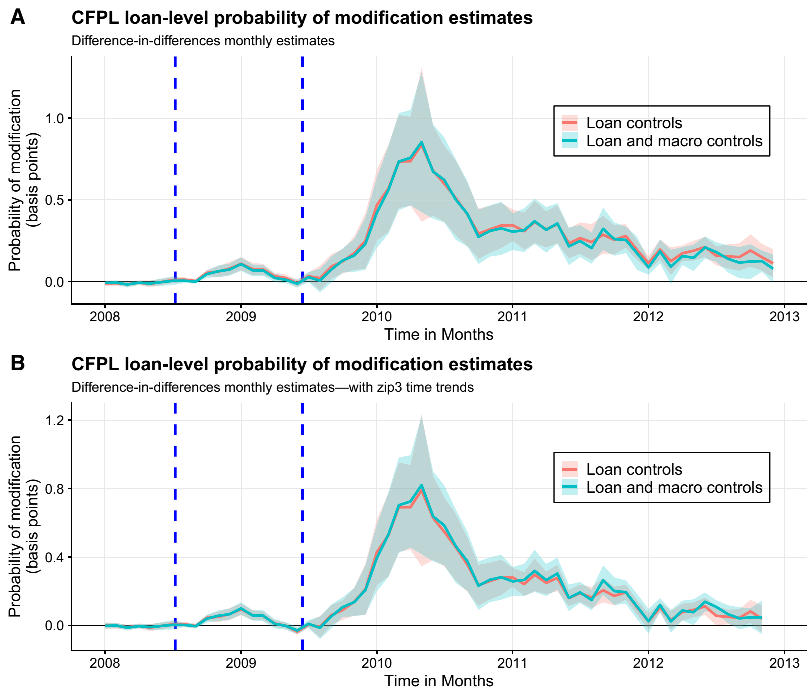

Figure 7
Loan-level modification rate difference-in-differences estimates
Loan-level modification rate difference-in-differences linear probability model regressions. The left-hand-hand side variable takes a value of 1 if a loan enters modification and zero otherwise. These regressions are based on 206,530,893 loan-month observations. For further information on model specification, see the notes to figure 3.

We plot the estimation output of θ_y from the previous equation in panel A of Figure 7. The vertical axis is in basis points. The path of θ_y shows that there is no pretreatment 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, Amromin, Chomsisengphet, et al. 2015; Agarwal, Amromin, Ben-David, et al. 2017)). Prior to the announcements of HAMP and HARP, SB-1137 increased the modification rate, relative to a counterfactual of non-California regions, by 38%.

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, Amromin, Chomsisengphet, et al. 2015; Agarwal, Amromin, Ben-David, et al. 2017), and thus the increase in modifications due to the CFPA preceded the implementations of the federal programs. Using data through the end of 2012, the estimated increase in the modification rate due to the CFPLs is 13.1 basis points. A back-of-the-envelope application of this estimate applied to all 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 one million modifications (Agarwal, Amromin, Ben-David, et al. 2017). Using the total number of housing units with a mortgage from the ACS Survey, the estimates imply that the CFPLs induced 68% of the modification increases relative to HAMP without any pecuniary subsidies.[27] Also, unlike the CFPLs, HAMP did not include any provisions to increase foreclosure costs.

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

4.1 Did the CPFLs increase the cure rate for mortgages in default?

While our previous analysis shows that the CFPLs led to an increase in mortgage modifications overall, an additional important question concerns the cure rates for mortgages in default. If REO foreclosures decline (as documented earlier), then mortgages can either linger in delinquency or cure (become current on mortgage payments).[28] Here we thus analyze the probabilities that 90-day-delinquent mortgages in the month prior to the CFPLs subsequently cured. We employ the same transition probability research design and data used in Equation 4. The results are in Figure 8, where the red-dashed vertical line indicates that delinquency was measured in the month prior to the CFPLs (June 2008), and the two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA, respectively.

First, there was no statistically significant difference in the probability that loans were current prior to the CFPLs, indicating that the parallel pre-trends assumption is satisfied. Next, with the implementation of SB-1137 in July 2008, there was a statistically significant uptick in the probability of transition from 90 days delinquent to current. Then, following the implementation of the CFPA in mid-2009, the cure rate increases markedly, reaching over 25 basis points at the end of 2010. Note that the overall path of the cure rate matches that from the modification rate estimates in Figure 7, in line with the cure rate for these mortgages being in part due to modification.

[27] Using the estimate that HAMP created 1 million modifications from Agarwal, Amromin, Ben-David, et al. (2017) and data from Table B25081 from the one-year 2007 ACS survey, the modification rate for HAMP was 1,000,000/51,962,570 = 0.019. In comparison, the modification rate computed 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 * 0.0131 = 70,502.55 modified California mortgages.

[28] A foreclosure alternative is also possible as discussed above as well as mortgage pre-payment. For other studies on cures in modification, see Adelino, Gerardi, and Willen (2013).

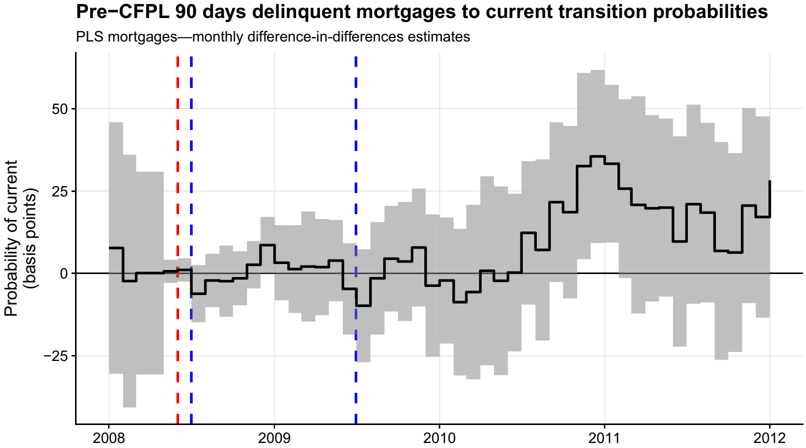

Figure 8
Cure rate: difference-in-differences transition probabilities from default to current
Loan-level year-month Moody's BlackBox private-label mortgage data using using private-label mortgage loans sold into private-label securitization (PLS). The red-dashed vertical line represents when delinquency status was measured, the month before the CFPL announcement in June 2008. The two blue-dashed vertical lines are the implementations of SB-1137 and the CFPA, respectively. Loan-level controls include three-digit zip code and time fixed effects; dummy variables for the origination year-month; indicator variables for contract loan type including whether or not the loan is a hybrid ARM, an option ARM, or a negative amortization mortgage; if it had a balloon payment, an interest-only period, and is an ARM loan that could be converted into a fixed rate loan; the origination balance; the FICO credit score and LTV at origination; dummy variables for the interest rate index for ARM loans with a separate variable for fixed rate loans; and fixed effects for the following variables: loan purpose, property type, and servicer. Data are from Arizona, California, and Nevada. Gray bands correspond to ±2 robust standard errors clustered at the three-digit zip code level.

5. 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.[29] 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 the 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 triple-differences design. First, we retain our synthetic control REO foreclosure gap estimates (Figure 2, panel 2), the

29 Campbell, Giglio, and Pathak (2011); Anenberg and Kung (2014); Gerardi et al. (2015); Fisher, Lambie-Hanson, and Willen (2015); Mian, Sufi, and Trebbi (2015).

difference-in-differences in foreclosures for each California county relative to their estimated 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 obtain the abnormal house price growth for each California zip code—analogous to an abnormal equity return—through synthetic control gap estimates.[30] For each California zip code, we apply the synthetic control 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 9, panel 1. The notes to Figure 9 list the variables used to build the zip code synthetic 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 9, 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 triple-differences CFPL estimates that measure the increase in house prices due to a decline in foreclosures. Using OLS, the slope is −0.023 (robust standard errors clustered at the three-digit zip code level: 0.004), while the median slope from a quantile regression that is robust to outliers is −0.027 (robust standard error: 0.002). Online Appendix J 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 (−0.027) and the median CFPL synthetic control gap decline in REO foreclosures per 10,000 homes (−307.29), CFPL REO foreclosure reduction increased housing returns for the median zip code by 8.29%. Applying the distribution of REO foreclosure quantile regression estimates across California implies that the CFPLs increased California aggregate house price returns by 5.4% ($300 billion).

Finally, Figure 9, panel 2B, 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 areas with large REO foreclosure reduction. For counties in the second quintile, for example, in terms of CFPL REO foreclosure reduction, abnormal house prices increased 13%. In areas with the minimal foreclosure change (for example, quintiles 3 and 4), there was little abnormal house price growth. Quantiles

30 Abnormal Return = Actual Return − Expected Return

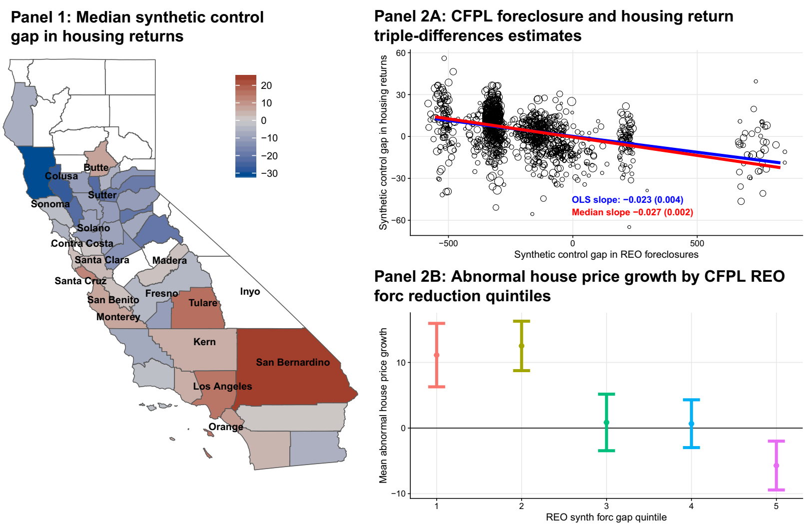

Figure 9
Zip code CFPL house price estimates
Panel 1 shows the median zip code level synthetic control gap in house price growth (%) within each California county from 2008M07 to 2011M12. For each California zip code, we construct a synthetic control using the following variables during the pretreatment period: housing returns; random forest 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 triple-differences 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 three-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 synthetic control foreclosure gap quartiles.

3 and 4 in panel 2B also constitute a falsification test: California housing markets with limited REO foreclosure reduction experienced no abnormal house price growth relative to controls, in line with CFPL foreclosure reduction generating abnormal California house price growth during the treatment period. In other words, California housing markets with no CFPL impacts were not different from controls during the treatment period. Finally, the outlier areas that experienced an increase in foreclosures also experienced a decline in house price returns.

6. Discussion and Other Results

Our synthetic control results suggest that the CFPLs prevented 250,000 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, Amromin, Chomsisengphet, et al. 2015; Agarwal, Amromin, Ben-David, et al. 2017).[31] 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, Matsa, and Melzer (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 to taxpayers compared with these other programs as they did not provide pecuniary subsidies to lenders and borrowers (HAMP/HARP) or to unemployed households (unemployment insurance).

6.1 SB-1137 versus the California Foreclosure Prevention Act (CFPA)

As noted earlier, as part of a larger and sustained effort to ameliorate crisis period foreclosures, California passed and implemented two foreclosure amelioration laws: SB-1137 in July 2008 and the California Foreclosure Prevention Act (CFPA) in June 2009. We collectively refer to these laws as the CFPLs (California Foreclosure Prevention Laws). As California implemented these laws within a limited timeframe, it is difficult to parse out their separate effects. Nonetheless, some discussion is in order. As SB-1137 was announced and implemented first, we clearly identify its large and immediate impact on foreclosures. Yet, the opportunity to identify the impacts of the CFPA, separate from SB-1137, is limited in that SB-1137 changed the path of California's housing market and there is thus no obvious counterfactual for California to independently identify the impacts of CFPA. Hence, we view sustained foreclosure reduction post-CFPA implementation holistically and as the combined result of the two policies. We leave further separate identification of the two policies as an avenue for future research. However, as noted by a referee and as stated previously, the increase in modifications due to the CFPA is pronounced and appears to be a direct effect of this policy.

6.2 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 (for example, 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 our analysis of the CFPLs during the recent crisis, Rucker and Alston find that the farm foreclosure moratoria reduced farm

[31] Numbers from Hsu, Matsa, and Melzer (2018) and the Mortgage Bankers Association.

foreclosures during the Great Depression. In other work, Pence (2006) and Mian, Sufi, and Trebbi (2015) study judicial and nonjudicial states before and during the crisis and conclude that the increased costs associated with judicial foreclosure limited foreclosure instantiation. While the CFPLs were similar in some aspects to the aforementioned policies, they were unique in their scope and implementation: the CFPLs encouraged modifications through increased foreclosure durations and incentivized foreclosure maintenance spending. 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.

6.3 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 (i) increasing interest rates on new mortgages to compensate for the depreciation of the foreclosure option or (ii) rationing credit, especially in environments where raising interest rates is infeasible.[32] For the CFPLs, (i) would translate into fewer loans being originated in California post-policy, ceteris paribus. With regard to (ii), Alston notes that during the Depression, lenders were reluctant to increase interest rates, as this would have created "hostility and ill will" (Alston 1984, 451). Similar concerns may have also deterred lenders from increasing interest rates in California following housing crisis.

Conversely, in its 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 earlier), then lenders may have viewed the CFPLs favorably as foreclosures can create deadweight losses for lenders (Bolton and Rosenthal 2002). Further, as the private-label mortgage backed security market collapsed following the Great Recession, the government-sponsored enterprises (GSEs) were the primary securitizers of residential mortgages, and GSE lending composed the majority of the mortgage market. As the GSEs do not discriminate based on geography (Hurst et al. 2016), we should expect their prevalence post–Great Recession to temper any credit rationing in response to the CFPLs.

In Online Appendix K, we employ the Home Mortgage Disclosure Act (HMDA) data set to determine the impact of the CFPLs on mortgage credit following the implementation of the policy. Overall, we find that California borrowers were not more likely to be denied credit and did not experience credit rationing in the aftermath of the CFPLs.

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32 Lenders ration credit as underwriting costs increase (Sharpe and Sherlund 2016).

6.4 Did the CFPLs induce strategic default?

As the CFPLs lowered foreclosures and increased modifications, an important issue for policymakers concerns strategic defaults, where borrowers intentionally miss payments in order to obtain a mortgage modification from their lender. It is important here to note that the CFPLs did not provide direct subsidies to borrowers, like the federal government's HAMP program, and thus incentives for strategic default may differ for the CFPLs relative to the federal programs.

Our strategic default estimation approach follows Mayer et al. (2014): to proxy for strategic default, we examine mortgages that roll straight from current to 90 days delinquent.[33] In other words, we examine the probability that a borrower misses three payments in a row, given that they were initially current, and hence yielding the following regression model that extends our previous analyses:

$$\begin{aligned}
&\text{Prob}(\text{90 Days Delinquency}_{it}|\text{Current}_{i,t-3}) \\
&= \sum_{\substack{y=1 \\ y\neq 2008\text{M}06}}^{T} (\theta_y \mathbf{1}\{y=t\} \times CA_i) \\
&\quad + \sum_{\substack{y=1 \\ y\neq 2008\text{M}06}}^{T} (\mathbf{1}\{y=t\} \times \mathbf{X}_i' \boldsymbol{\lambda}_y) + \delta_t + \text{zip3}_i + \varepsilon_{it} \qquad (9)
\end{aligned}$$

The results are in Figure 10.[34] The blue-dashed vertical line represents the announcement and implementation of SB-1137, and the green-dashed vertical line signifies the date when loans that were current prior to the CFPLs could first be 90 days delinquent.

Figure 10 shows that prior to the CFPLs, there was no difference in the propensity for strategic defaults between California and non-California borrowers, and hence the parallel pre-trends assumption is satisfied. Then, immediately following SB-1137, the relative probability that a mortgage transitioned straight from current to 90 days delinquent dropped, highlighting the efficacious policy effects for borrowers that were current just prior to the CFPLs. Next, as noted by the green line, for the cohort that was current just prior to the CFPL announcement in June 2008, the probability of transitioning straight to 90 days delinquent fell further. This latter evidence is counter to the notion that borrowers strategically exploited the CFPLs to obtain modifications.

[33] See also Artavanis and Spyridopoulos (2018).

[34] The gray bands correspond to ± 2 robust standard errors clustered at the three-digit zip code level, and the loan-level controls, whose coefficients vary flexibly with time, are listed in the Figure notes.

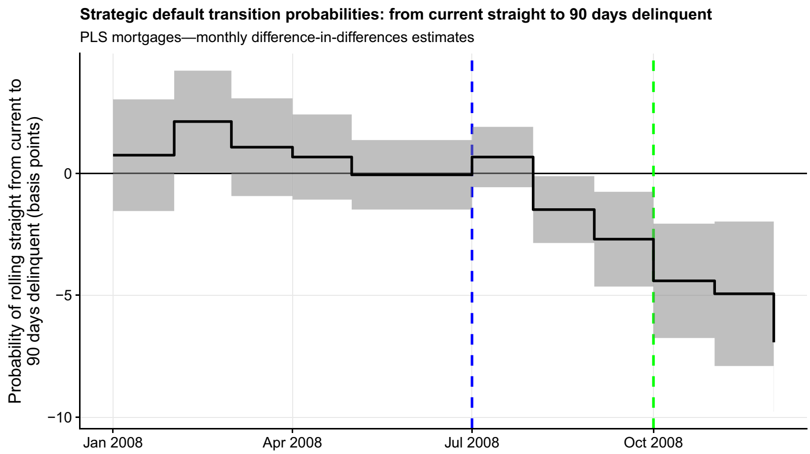

Figure 10
CFPL strategic default analysis
Loan-level year-month Moody's BlackBox private-label data. The blue-dashed vertical line is the announcement and implementations of SB-1137. The green-dashed vertical line is the month where loans that were current (no missed payments) prior to the announcement of the CFPLs (June 2009) could have strategically defaulted. Loan-level controls include zip code and time fixed effects; dummy variables for the origination year-month; indicator variables for contract loan type including whether or not the loan is a hybrid ARM, an option ARM, or a negative amortization mortgage; if it had a balloon payment, an interest-only period, and an ARM loan that could be converted into a fixed rate loan; the origination balance; the FICO credit score and LTV at origination; dummy variables for the interest rate index for ARM loans with a separate variable for fixed rate loans; and fixed effects for the following variables: loan purpose, property type, and servicer. Data are from Arizona, California, and Nevada. The gray bands correspond to ±2 standard errors clustered at the three-digit zip code level.

7. Conclusion

In this paper, we estimate the impacts of the California Foreclosure Prevention Laws, financial crisis period interventions that enabled mortgage foreclosure abatement and forbearance for distressed borrowers in the nation's largest housing market. Our results show that the CFPLs prevented 250,000 REO foreclosures and created \$300 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 100,000 REO foreclosures and created \$70 billion in housing wealth.

Policies aimed at keeping distressed mortgage borrowers in their homes represent a common thread across economic and financial crises. Our CFPL findings may thus serve as a guide to policymakers, while other instances of foreclosure abatement and mortgage forbearance may provide an opportunity to assess the external validity of our CFPL policy response. For example, Rucker and Alston (1987) find that foreclosure moratoria reduced farm foreclosures during the Great Depression. More recently, to combat the COVID-19 induced economic crisis, the U.S. government implemented mortgage forbearance through the CARES Act. The study of this wide-reaching COVID-19 mortgage forbearance program allows for further evaluation of a CFPL-like policy intervention and represents an excellent avenue for further research.

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