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

Stuart Gabriel
University of California, Los Angeles

Matteo Iacoviello
The Federal Reserve Board

Chandler Lutz
Copenhagen Business School

February 27, 2017

Abstract

This paper investigates the housing and broader economic effects of the 2000s crisis-period California Foreclosure Prevention Laws (CFPLs). The CFPLs encouraged lenders to modify mortgage loans by increasing the required time and pecuniary costs of foreclosure. We find that the CFPLs prevented 380,000 California foreclosures, equivalent to a 16% reduction during the treatment period. These effects did not reverse after the conclusion of the policy, implying that the CFPLs were *not* a stopgap measure that simply pushed foreclosures further into the future. Our most conservative results show that these policies increased house prices by 6 percent and in doing so created $300 billion of housing wealth. Findings further indicate that gains in housing wealth translated into increased durable consumption as measured by auto sales. Disaggregated county and zip-code level estimates reveal that the CFPL house price increases were markedly higher in the hard hit areas of Southern California. Altogether, results suggest that the CFPLs were substantially more effective than the US Government's HAMP Program in mitigating foreclosures and stabilizing housing markets.

JEL Classification: E52, E58, R20, R30;

Keywords: Foreclosure Costs, Mortgage Modification, Housing Crisis

*We would like to thank Juan Pablo Chauvin, Erik Hambre, Joe Tracy, Paul Willen, Kerry Vandell, and numerous seminar participants for their helpful comments. Gabriel: stuart.gabriel@anderson.ucla.edu. Iacoviello: matteo.iacoviello@frb.gov. Lutz: cl.eco@cbs.dk. Gabriel acknowledges funding from the UCLA Gilbert Program in Real Estate, Finance, and Urban Economics. Lutz acknowledges funding from the UCLA Ziman Center for Real Estate's Howard and Irene Levine Program in Housing and Social Responsibility. Code available at https://github.com/ChandlerLutz/CFPLCode and data available at https://github.com/ChandlerLutz/CFPLData.

At the height of the housing boom in 2005, California accounted for one-quarter of US housing wealth.[1] But as boom turned to bust, house prices in the state fell by 30 percent and over 800,000 California homes entered foreclosure.[2] In an effort to contain mounting foreclosures both in California and beyond, the Federal Government enacted the Home Affordable Modification Program (HAMP) to incent widespread modification by offering financial subsidies to both homeowners and lenders.[3] However, HAMP had little economic impact as certain mortgage lenders lacked the infrastructure to modify loans on a large scale (Agarwal et al., 2017).[4] At the epicenter of the housing bust, the State of California pursued an alternative policy strategy to aid distressed borrowers and limit substantial foreclosures. In contrast to the US Government approach of offering financial incentives to modify individual loans, California instead imposed foreclosure moratoria and increased foreclosure pecuniary costs to facilitate widespread lender adoption of mortgage modification programs. Thus in the California policy response, distressed borrowers received policy treatment even in the event of inaction by their lenders. Unlike HAMP, there has been little focus on and no prior evaluation of such alternative policy efforts that increased foreclosure costs to stem the 2000s housing crisis. In this paper, we undertake such an evaluation and use California as a laboratory to measure the housing and broader economic effects of the California Foreclosure Prevention Laws (CFPLs).

California is a non-judicial foreclosure state. Prior to the enactment of the CFPLs, the state only required a lender initiating a foreclosure to deliver a notice of default (NOD; foreclosure start) to the borrower by mail. A 90-day waiting period then commenced before the lender could issue a notice of sale (NOS) of property. In July 2008 and in the midst of a severe housing crisis, the state passed the first of the CFPLs, Senate Bill 1137 (SB-1137).[5] This bill, which immediately went into effect, prohibited mortgage lenders and servicers (henceforth, lenders) from issuing a NOD until 30 days after informing the homeowner of potential foreclosure alternatives either by telephone or in person.[6] The homeowner then had the right within 14 days of first contact to schedule a second telephonic meeting with the lender to discuss foreclosure

[1]Number of housing units by state from table S1101 of the 2005 American Community Survey. State-level house prices are from Zillow in 2005.

[2]The foreclosure rate is from the Mortgage Bankers' Associations.

[3]The Federal Government also implemented other housing policy during the crisis. These programs are discussed below in section 1.

[4]Specifically, Agarwal et al. (2017) find HAMP reached just a third of the targeted 3-4 million households. For other studies on HAMP see Hembre (2014) and Scharlemann and Shore (2015).

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

[6]If lenders could not reach homeowners, they had to undertake "due diligence" in their attempts to contact the homeowner. See section 1 for more details.

alternatives. SB-1137 additionally mandated that agents who obtained a vacant residential property through home foreclosure must maintain the property or face fines of up to $1000 per day, further increasing lender out-of-pocket foreclosure costs. In the second CFPL wave, California passed the California Foreclosure Prevention Act (CFPA) in early 2009. The CFPA prohibited mortgage lenders from sending borrowers a NOS for an *additional 90 days* subsequent to the issuance of the NOD unless the lender had implemented a broad-based mortgage modification program. The adequacy of mortgage modification programs was determined by the State of California based on debt-to-income targets and potential interest rate or principal payment reductions.[7] Therefore, like SB-1137, the CFPA extended the duration and pecuniary costs of foreclosure in an effort to encourage widespread mortgage modification and limit the ongoing mortgage default crisis.

The CFPLs were unique in their scope and intervention. Further, they were implemented at a moment when prices in many California housing markets were spiraling downward. As such, these policies provide a rare opportunity to assess the housing and related economic effects of important crisis-period policy interventions that sought to encourage widespread mortgage modification. Our most conservative estimates show that the CFPLs reduced Real Estate Owned (REO) foreclosures by 16 percent and hence prevented 380,000 California borrowers from losing their homes. The CFPLs also mitigated prime and subprime foreclosure starts and reduced household mortgage default risk.

Those same conservative estimates show that the relative gain in California house prices due to the CFPLs was 6 percent – equivalent to a $300 billion increase in housing wealth.[8] Our median and preferred estimate of the house price appreciation associated with the CFPLs, derived using highly disaggregated zip code level data, is 9 percent. These effects were largely concentrated in the hard-hit areas of Southern California where the CFPLs dramatically lowered foreclosures. Indeed, foreclosure reduction is the key channel by which the CFPLs affect house prices, and using zip code level data we find that the CFPLs caused a 14.7 percent relative house price increase in the Southern California Inland and Coastal regions.

To put the CFPL house price gains into perspective, note that the effective US Government fiscal stimulus during the crisis, through the American Recovery and Reinvestment Act of 2009

[7] See section 1 for more details.

[8] According to table S1101 of the 2007 1-Year ACS Community Survey, there were 12,200,672 homes in California is 2007. The median house price in 2008M06 according to Zillow was $413,000. Thus, $413,200*12,200,672*0.06 $\approx$ $302 billion.

(ARRA) and social transfers, totaled \$114 billion.[9] The magnitude of the housing stimulus created by the CFPLs (\$300 billion using our most conservative estimate) was thus 260 percent of the effective US Government package. This implies that our CFPL estimates are large in magnitude, economically meaningful, and highlight how the CFPLs ameliorated the ailing California housing markets during the policy period.

The CFPLs not only lowered foreclosures, but also increased mortgage modifications. Using a difference-in-differences research design and loan-level data, we find that the modification rate for delinquent loans in California increased 0.5 percentage points – a 29 percent relative increase – due to the CFPLs. These estimated effects are large in magnitude, economically meaningful, and robust to the inclusion of loan-level characteristics, as well as housing market and macroeconomic indicators as controls.

A priori, the housing market effects of the CFPLs were uncertain. Larry Summers, the Director of the National Economic Council during the crisis, noted that the Federal Government elected not to increase foreclosure durations as any such increase would simply delay foreclosures until a later date.[10] This was the prevailing view among leading US policymakers during the crisis.[11] On the other hand, prior academic research provides a basis through which the CFPLs may affect housing markets. First, Pence (2006) notes that judicial foreclosure laws – laws that mandate that lenders must process foreclosures in state courts – increase both the costs and duration of the foreclosure process. Building on this observation, Mian et al. (2015) find that states with a judicial foreclosure requirement experienced markedly lower rates of foreclosure and relatively higher house prices during the 2000s housing crisis.[12] The economic rationale for house price gains in areas with lower foreclosure rates is based on foreclosure neighborhood externalities or theories of foreclosure induced fire sales. With regard to foreclosure externalities, a large literature contends that foreclosures negatively affect nearby house prices (the so-called foreclosure spillover) by increasing housing supply (Anenberg and Kung, 2014) or through a "disamenity" effect where distressed homeowners neglect maintenance of

[9] Oh and Reis (2012) find that the increase in discretionary transfers from 2007-2009 was \$96 billion (see also Kaplan and Violante (2014)), while Cogan and Taylor (2013) find that only \$18 billion of ARRA stimulus was spent for federal purchases. The remainder of ARRA funds were granted to states who subsequently reduced their borrowing. The total effective discretionary fiscal increase from 2007-2009 was \$114 billion.

[10] "Lawrence Summers on 'House of Debt' ". *Financial Times.* June 6, 2014. Note that Summers did not discuss the potential effects of programs that increased both costs and durations of foreclosures.

[11] See, for example, "Geithner Calls Foreclosure Moratorium 'Very Damaging' ". *Bloomberg News.* October 10, 2010.

[12] Goodman and Smith (2010) also find that states with lower default rates also placed higher pecuniary and time foreclosure costs on lenders.

their homes (Gerardi et al., 2015).[13] During a foreclosure induced fire sale, a downward house price trend may reverse if the frequency with which houses become available for sale slows (Mian et al., 2015).[14] Hence by increasing the duration and cost of the foreclosure process, the foregoing academic studies imply that CFPLs could have had a positive effect on housing markets if these laws reduced the flow of homes entering the foreclosure process. This is what we find in our empirical work: The CFPLs lowered mortgage defaults while increasing modifications and thus damped the downturn in housing, suggesting that an increase in mandated foreclosure costs is effective in buttressing ailing housing markets. Further, in contrast to the views of Summers and other leading policymakers, we find no evidence that policy effects later reversed as the CFPLs did not induce lingering delinquencies, prolong the crisis, or simply delay foreclosures until a later date. In other words, the salutary effects of the CFPLs were not transitory.

A further concern raised by policymakers and others was that housing interventions such as the CFPLs might hamper future lending owing to changes in the terms of mortgage default and foreclosure.[15] Using the loan-level HMDA dataset, we find that the CFPLs created no adverse side effects for new borrowers in terms of the probability of mortgage application denial and did not limit the flow of credit to California.

In addition to bolstering housing markets, the CFPLs were broadly beneficial to the real economy. Specifically, we find that these policies increased durable consumption as measured by auto sales. Compared to an estimated counterfactual, California auto sales increased 12 percent; further, growth in auto sales was highest in areas where the CFPLs were most efficacious. Indeed, we estimate an elasticity of CFPL house price growth for auto sales growth of 0.29, in line with findings from previous research.

The broader economic impacts of the CFPLs were also unclear ex ante as there has been little evidence regarding the marginal propensity to consume (MPC) out of an increase in housing wealth during a severe housing downturn. Tim Geithner, the US Treasury Secretary from 2009-2013, contended that the MPC out of an increase in housing wealth during the crisis was near zero (Geithner, 2014). This line of thinking postulates that households are unwilling or unable to increase consumption simply because the decline in the value of an already highly

[13] See also Lambie-Hanson (2015) and the references therein.

[14] See also Shleifer and Vishny (1992), Kiyotaki and Moore (1997), Krishnamurthy (2003), and Lorenzoni (2008).

[15] In the literature, there is debate on this point. See, for example, Alston (1984) and Bolton and Rosenthal (2002). "Lawrence Summers on 'House of Debt' ". *Financial Times*. June 6, 2014.

depreciated asset is less steep. In contrast, recent academic research undertaken both prior to and in the aftermath of the crisis estimates the MPC out of housing wealth at 0.05 to 0.10.[16] Increases in housing wealth may affect consumption through a wealth channel or a credit constraints (refinancing) channel.[17] We document evidence in support of the credit constraints channel. In particular, we find that CFPL house price growth generated higher refinancing volume and hence that the CFPLs eased credit conditions for California households.

1 The California Foreclosure Prevention Laws (CFPLs)

The State of California sought to mitigate the effects of the 2000s housing crisis first through SB-1137 in July 2008 and then again with the passage of the CFPA in February 2009 (implemented in June 2009). The CFPLs aimed to incent mortgage lenders to modify loans by increasing the pecuniary and time costs of foreclosure.

1.1 California Senate Bill 1137 (SB-1137)

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

Prior to the enactment of SB-1137, existing law only required that the lender file a NOD with

[16] See Bostic et al. (2009) and Mian et al. (2013) for an overview.

[17] Mian and Sufi (2014) also show how changes in housing net worth affected non-tradable employment during the crisis.

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

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

the appropriate county recorder and then mail the NOD to the mortgage borrower. In sending the NOD, lenders were not obligated to provide information on foreclosure alternatives. The aim of SB-1137 was to alert struggling homeowners of foreclosure alternatives via mortgage lenders. Indeed, the Bill's chaptered text cites a Freddie Mac report that suggested that 57 percent of late paying borrowers did not know that their lender may offer a foreclosure alternative. Further, by increasing the costs of foreclosure, the State of California sought to change the net present value calculation of foreclosure versus mortgage modification.

1.2 The California Foreclosure Prevention Act (CFPA)

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

Mortgage lenders who implemented an acceptable mortgage modification program were exempted from the additional 90 day CFPA foreclosure moratorium. To obtain this exemption, a lender's loan modification program was required achieve affordability and sustainability targets for modified loans. Note that lenders participating in the HAMP program were considered to be in compliance with the CFPA and thus were exempt from the extra 90 day foreclosure moratorium under the law. The eligibility, affordability, and sustainability targets of the CFPA as well as the exact CFPA timeline are discussed in appendix C.

In total, 149 applications were submitted for exemptions from the CFPA foreclosure moratorium. Of these 149 applications, 78.5 percent were accepted, 12 percent were denied, and 10 percent of the applications were withdrawn. Hence, a non-trivial portion of the submitted mortgage modification programs did not meet the CFPA standards. Of the 117 accepted applications, only 31 lenders obtained an exemption from the CFPA via the US government's HAMP program; indicating that the vast majority of lenders were not participating in the federal program and thus the CFPA may have provided stronger incentives for lenders to implement a mortgage modification program.

While accurate data on mortgage workouts under the CFPA from the state government is scant (California, 2010), surveys from lenders suggest that a large number of loans were modified under the CFPA: Permanent mortgage modifications totaled over 171,000 from 2009Q3 to 2010Q3 and represented nearly one-third of the mortgage workouts closed over that time period.[20] In comparison, the average number of loans in foreclosure across quarters was approximately 120,700 and thus the extent of the mortgage modifications appears to be large in magnitude and economically meaningful. Of the approximately 171,000 permanently modified loans, about 110,000 of these mortgages were modified outside of HAMP. Hence, HAMP accounted for just 35 percent of the modified mortgages over the foregoing period. Interestingly, Agarwal et al. (2017) find nationally that HAMP reached just one-third of its targeted homeowners, implying that the additional mandated requirements of the CFPA may have allowed modifications in California to reach the levels targeted by the federal program.[21] Below in section 7, we use Fannie Mae Loan Performance data to further examine loan modifications under the CFPLs.

Finally, lenders regulated by the California Residential Mortgage Lending Act (CRMLA) who received an exemption under the CFPA handled just 65.5 percent of the total CRMLA mortgage servicing volume in 2008. This suggests that a substantial number of CRMLA mortgages fell outside CFPA mortgage modification programs and thus were subject to the additional 90 day CFPA foreclosure moratorium in the event of default. Last, California (2010) notes that number of applications for the CFPA exemption was lower than anticipated as some lenders preferred the additional 90 days in foreclosure so they could avoid taking possession of non-performing properties during the height of the foreclosure crisis.

2 Data

We undertake analyses of the effects of the CFPLs on housing and related markets at the state, county, and zip code levels of geography. More aggregated data, for example at the state-level, allow us to consider a wide range of variables. Disaggregated data are also advantageous given

[20] Survey data are tabulated in (California, 2010). Other mortgage workouts resulted, for example, in the account being paid current, a short sale, or the account being paid-in-full.

[21] The 171,000 permanent modifications aided borrowers in the following ways: 113,733 resulted in monthly payment reductions, 82,864 extended the original loan term to no more than 40 years, 60,932 reflected principal payment reductions, and 30,202 deferred principal until maturity. See California (2010) for more details.

the breadth of California and the substantial heterogeneity among local California housing markets. Indeed, more local data allow us to estimate differing local effects of the CFPL policies, control for local housing and economic conditions, as well as use a larger the number of cross-sectional observations to improve the power of our statistical tests. Our sample period ranges from the start of 2004 through the end of 2014. The data and original links are available online.[22]

Mortgage Bankers Association (MBA) Data: At the state-level, the MBA provides data on foreclosure starts (NODs). In our main analysis, we consider MBA foreclosure starts, as a percentage of loans for (1) all loans, (2) only prime loans, and (3) only subprime loans. The MBA data are quarterly. Below, we also use MBA series that track the percentage of loans that are 60 days delinquent, the percentage of loans that are 90 days delinquent, the percentage of loans that are seriously delinquent (more than 90 days delinquent), and the so-called foreclosure inventory (the percentage of loans in foreclosure; the stock of foreclosures).

Zillow Data: From Zillow, we obtain real-estate owned (REO) foreclosures, at the state and counties levels, as well as hedonic house price indices at the state, county, and zip code levels. We use the All Homes (median), Bottom Tier (bottom third), and Top Tier (top third) house price indices. The Bottom and Top tier indices are not available at the zip code level.

FHFA Data: Our state-level analysis also employs the repeat-sales FHFA house price indices. The FHFA data only use house prices based on conforming mortgages sold to GSEs.

House Price Transformations: We transform all house price series using the log-first difference to obtain a housing return. As controls we also compute the house price growth and housing return variance in the pre-CFPL period (2004M01-2008M06; 2004Q1-2004Q2) and the house price growth one year before the CFPL treatment.

Mortgage Default Risk (MDRI): Our state-level dataset also includes the Mortgage Default Risk Index of Chauvet et al. (2016) to gauge household mortgage distress.

HMDA Data: Using HMDA data, we examine mortgage application denial at the loan-level and mortgage volume growth at the zip code level. We also retain other potential controls, such as applicant income, from the HMDA dataset.

Fannie Mae Loan Performance Data: To study the impact of the CFPLs on mortgage modifications, we use Fannie Mae Loan Performance Data. In addition to information on mortgage modifications, this dataset reports key borrower characteristics including current

[22]https://github.com/ChandlerLutz/CFPLData

delinquency status as well as the credit score, the debt-to-income ratio, and the interest rate at origination. Each loan is followed monthly while it remains in the Fannie Mae loan portfolio.

Auto Sales: To assess the impact of the CFPLs on the real economy, we use county-level, quarterly auto sales from RL Polk (the lowest level of aggregation with which we have access). These data are widely used in the literature as a proxy for durable consumption.

Other Macro Data: We also tabulate a large macro dataset to use as controls. At the state-level, we obtain population, unemployment, and median income estimates from the FRED database. Using ACS data, we compile information on median income, population and housing units. County-level unemployment rates are from the Bureau of Labor Statistics. At the zip code level, measures of population, number of households, and household income are obtained from the IRS Statistics of Income. Shapefiles and land area information were downloaded from the US Census. Finally, the Missouri Data Bridge is used to link data across geographies.

3 Methodology: Difference-in-Differences and Synthetic Control

To assess the impact of the CFPLs, we employ both a difference-in-differences (diff-diff) research design and the Synthetic Control Method (SCM) of Abadie et al. (2010) and Abadie and Gardeazabal (2003).[23] The diff-diff approach has been used throughout the housing literature to analyze mortgage modification programs and related policies,[24] but the choice of a comparison group within the diff-diff approach is difficult and "ad-hoc" (Peri and Yasenov (2015) and Card (1990)). Thus we also implement the SCM as it employs a data-driven algorithm to select an optimal control from a set of potential candidates not exposed to the treatment. For example, in our state-level analysis, we use the SCM to develop a "Synthetic California," an optimal linear combination of other states, whose key housing aggregates can then be compared to the actual values from California. At more disaggregated levels, we extend the SCM to identify separate policy estimates for individual California regions.

The diff-diff and SCM approaches both have their advantages and disadvantages. The diff-diff method is straightforward and robust to large datasets, but requires the researcher to subjectively identify the control group. In contrast, the SCM generalizes the usual diff-diff estimator to allow unobserved confounding characteristics to vary over time, uses data-driven techniques to identify the optimal control, and allows us to identify localized policy estimates.

[23] See also Abadie et al. (2011), Billmeier and Nannicini (2013), and Acemoglu et al. (2016).

[24] For recent examples, see Mayer et al. (2014), Agarwal et al. (2015), Agarwal et al. (2017).

The SCM, however, is computationally infeasible for extremely large datasets and better suited for aggregated data (the lowest level of data aggregation that we consider within the SCM is at the zip code level; in models below that use loan-level data, we only employ a diff-diff design). Yet most importantly, our results are robust to the use of these different methodologies and hence our findings do not hinge on a single econometric technique.

We define a Synthetic Control as a linear combination of potential controls that approximates the most pertinent characteristics of the treated unit (Abadie et al., 2010). Suppose that we observe $j = 1, \ldots, J+1$ units for $t = 1, \ldots, T$ time periods.[25] Without loss of generality, suppose further that the first unit is exposed to the treatment so that the remaining $j = 2, \ldots, J+1$ control units are available in the so-called "donor pool." In our case, the intervention commences with the passage of the CFPLs. Assume intervention occurs at time $T_0 + 1$; the pre-intervention period is $t = 1, \ldots, T_0$ and the post intervention period is $t = T_0 + 1, T_0 + 2, \ldots, T$.

Next, define two potential outcomes: (1) Let Y_{it}^N be the outcome for unit i in the post intervention period if i was *not* exposed to the intervention; and (2) let Y_{it}^I be the outcome for unit i if i was exposed to the treatment. Our goal is compute $\alpha_{1t} = Y_{1t}^I - Y_{1t}^N$ for periods $t = T_0 + 1, T_0 + 2, \ldots, T$, the causal impact of the intervention for the treated unit. As Y_{1t}^N is not observed, we need to construct a reasonable approximation for this missing potential outcome. In a diff-diff approach, the researcher subjectively selects elements of the donor pool for Y_{1t}^N. Oppositely for a Synthetic Control, let U_i be an $(r \times 1)$ vector of covariates for each i. U_i can include time varying or time invariant variables. The aim of the SCM is to select weights $W^* = (w_2^*, \ldots, w_{j+1}^*)'$, where $w_j^* \geq 0$ and $w_2^* + \cdots + w_{J+1}^* = 1$ for $j = 2, \ldots, J+1$, such that

$$\sum_{j=2}^{J+1} w_j^* \bar{Y}_j = \bar{Y}_1 \tag{1}$$

and

$$\sum_{j=2}^{J+1} w_j^* U_j = U_1 \tag{2}$$

hold (or hold approximately), $\bar{Y}_j = \sum_{s=1}^{T_0} \frac{1}{T_0} Y_{js}$, and $\bar{Y}_j$ is the average over pre-intervention outcomes.[26] The advantage of this approach is that it generalizes the diff-diff estimator as

[25] At more disaggregated levels, j can have multiple observations. In this case the usual diff-diff approach is used, while we apply the SCM to each element of j.

[26] See Abadie et al. (2010) for the more general case where multiple pre-intervention linear combinations are used.

linear combinations of pre-intervention outcomes can be used to control for unobserved common factors that vary over time.

In practice, typically there is no set of weights such that equations 1 and 2 hold exactly, so we follow Abadie et al. (2010) and choose the Synthetic Control unit that minimizes the distance between the characteristics of the treated unit and the convex hull of the control units. Specifically, we choose the W^* that minimizes

$$||X_1 - X_0W||_V = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)} \tag{3}$$

where $X_1 = (U_1', \bar{Y}_1)'$ is the characteristics of the treated unit, X_0 is a $((r+1) \times J)$ matrix of characteristics for the control units whose j-th row is $(U_j', \bar{Y}_j)'$, and V is an $(r+1) \times (r+1)$ symmetric and positive semi-definite matrix. An algorithm chooses V such that the mean-squared prediction error (MSPE) is minimized over the pre-intervention periods.

To conduct inference within the SCM, we implement placebo tests where the intervention is assigned to the control units that were not exposed to the treatment. The rarity and magnitude of the intervention on the treated unit is then compared to this set of placebo effects. In our application, the treatment is iteratively assigned to each member of the donor pool, forming a permutation test. A large and rare estimated treatment effect, relative to the distribution of placebo effects, supports a causal interpretation of results.

4 Sand States Difference-in-Differences Analysis

We first undertake a diff-diff analysis of the CFPL housing market effects. Our control group for California is the other Sand States – Arizona, Florida, and Nevada. The Sand States comprise a natural control group as these states all (1) experienced a substantial boom in house prices during the 2000s; (2) suffered high default rates and plummeting house prices during the housing bust; and (3) are often grouped together in descriptions of the excess that transpired during 2000s housing boom.

Figure 1 plots total, prime, and subprime foreclosure starts as a percentage of outstanding loans within each category; Zillow REO foreclosures per 10,000 people; the growth in household mortgage default risk (MDRI); and housing returns (FHFA and Zillow) for the Sand Sates from 2004 through the end of 2014. In each plot, the path of the variable for California is the black-bold line, the other Sand States are the blue lines. We denote the passage of SB-1137 in 2008Q3 (2008M07) with the long-dashed-red vertical line, the implementation of the CFPA in 2009Q2 (2009M06) with the short-dash-green vertical line, and the sunset date for the CFPA (the end of our policy analysis period) with the two-dashed pink vertical line in 2010Q4 (2010M12). The policy period of interest ranges from the announcement of SB-1137 in July 2008 through the sunset of the CFPA in December 2010. Yet we show the path of all variables through the end of 2014Q4 (2014M12) to determine if there is any reversal in the policy effects after the conclusion of the CFPA.

First, the Sand States yield an apt comparison group for California during the pre-treatment period (prior to the passage of SB-1137; the long-dashed-red vertical line). Indeed, the pre-treatment foreclosure and housing return variables move in lockstep across the Sand States and the cross-state pre-treatment correlations in these variables are all near 1. Given the pre-treatment similarities of the Sand States, Arizona, Florida, and Nevada provide a fitting comparison group for California in a diff-diff analysis of the CFPLs.

Figure 1 also highlights the large and immediate effects of the CFPLs following their introduction in 2008Q3: In contrast to the other Sand States, California foreclosure starts fell markedly and thus the increased costs created by SB-1137 muted the ascension of mortgage defaults in California. From there, the implementation of the CFPA (2009Q2) further damped the growth of California foreclosure starts relative to the other Sand States. Panels 1B and 1C of figure 1 document that the effects of CFPLs benefited mortgagors across the credit distribution, permeating through both the prime and subprime markets.

In addition to damping foreclosure starts, the CFPLs had a notable impact on REO foreclosures and the growth in household mortgage default risk (the MDRI). Panel 2A documents that Sand State REO foreclosures rocketed upwards beginning in 2006 (Zillow does not report REO foreclosures for FL; so panel 2A only shows AZ, CA, and NV). Then in 2008Q3, California passed SB-1137 and experienced a palpable fall in REO foreclosures compared to the other Sand States, whose foreclosure rates remained elevated until 2011. Similarly, the MDRI in panel 2B shows that household mortgage default risk in California accelerated until the introduction of SB-1137 in 2008Q3. Then household mortgage default risk in California fell quickly, especially compared to the other Sand States.

With regard to prices, panels 2C through 3C indicate that California housing returns marched upwards over the CFPL period with the most notable deviations from the other Sand States beginning in 2009Q1, after the reduction in foreclosures began to permeate through housing markets. Panels 2C through 3C further show that increases in prices are robust to different house price methodologies (repeat-sales used by FHFA versus hedonic prices used by Zillow) and spread to different sectors of the housing market. In particular, panels 3B and 3C document a notable an uptick in returns for both Bottom and Top Tier homes with an especially large jump for houses priced at the bottom end of the market.

Overall, the path of the housing variables after 2008Q3 highlights the comparable improvement in California, and that the CFPLs therefore led to a broad-based improvement in housing markets. Finally, there is no reversal in trend of these housing market variables, meaning that the CFPLs effects were long lasting and did not dissipate after the end of the policy period.

Table 1 presents a summary of the change in housing markets at the state-level for the key variables highlighted in figure 1 during the CFPL treatment period (2008Q3-2010Q4). House prices and the MDRI are the change in logs over the treatment period; all other variables are the cumulative sum of the levels. Column (5) shows the diff-diff means estimate. The introduction of the CFPLs coincided with a dramatic relative improvement in California. During the CFPL treatment period there is a large relative reduction in the portion of homes that entered the foreclosure process via a foreclosure start (8.65 percentage points) and a notable relative drop in the number of REO foreclosures per 10,000 people (390.70). We also see similar declines in the MDRI. As shown in panel B, California state-wide house prices fell between 20.19 (FHFA) and 22.03 (Zillow) percent during the CFPL period, whereas the smallest house price drop among the other Sand States was the 30.96 percent decline in Florida measured using the FHFA data. The corresponding diff-diff means estimate for California house price growth is thus large in magnitude and ranges from 13.16 to 14.28 percent. The last two rows of Panel B suggest that the beneficial CFPL effects extend to both the Top and Bottom Tiers of the housing market.

We also conduct the diff-diff analysis at the county and zip code levels, which increases the number of cross-sectional observations and allows us to control for local housing and macroeconomic conditions during the pre-treatment period. In particular, in our diff-diff regressions we control for pre-treatment period (2004Q1-2008Q2) house price growth and housing return variance, house price growth one year prior to the treatment, and the unemployment rate, household median income, and population density in 2007. These controls account for local housing market pre-treatment growth and volatility as well key macroeconomic indicators. All of the regressions in this paper our weighted by the number of households in 2007. The results are in table 2. Within each panel, we list state-level weighted means in columns (1) - (4) and the diff-diff estimates are in column (5). White standard errors are in parentheses.

Panel A shows the change in county-level REO foreclosures over the CFPL period. California experienced notably lower REO foreclosures compared to the other Sand States, in line with a decline in defaults following the introduction of the CFPLs. The diff-diff estimate in panel A column (5) emphasizes the remarkable reduction in California REO foreclosures: With the passage of CFPLs there were 225 fewer REO foreclosures per 10,000, an estimated reduction in California foreclosures of nearly 30 percent. This estimate is significant at the 1 percent level. Panel B reports house price growth at the zip code level, while panel C shows these results at the county level. When available, we prefer the highly disaggregated zip code level data (panel B) as there is substantial within county variation in housing markets and other key variables such as income. Zip code analyses also yield a larger number cross-sectional observations and thus increase the power of our statistical tests. Panels B and C both indicate that the reduction in California foreclosures translated into large house price gains. Using the disaggregated zip code data in panel B, we find that the CFPLs led to a statistically significant 8.5 percent increase in California house prices. The county house price growth results in panel C confirm the zip code level findings, but also document an outsized CFPL impact on Bottom Tier homes.

Below we use the SCM to build alternative estimates, but the results from this diff-diff analysis are clear: The CFPLs attenuated the decline in the California housing market, resulting in lower foreclosures and higher house price growth.

5 Synthetic Control Housing Market Results

We use the SCM to estimate the causal impact of the CFPLs at the state, county, and zip code levels. The state-level results yield broad estimates of the CFPLs across California, while the county and zip code level findings describe the heterogeneous geographic impacts of the policy. For the county and zip code level analysis, we iteratively apply the SCM and build a Synthetic Control for each region in California.

5.1 State-Level Synthetic Control Results

At the state-level, outcome variables are identical to those used above in our diff-diff analysis. For each of these variables, we search for a Synthetic match using the following predictors:

Housing returns, pre-treatment period (2004Q1 - 2008Q2) house price growth and housing return variance, house price growth in the year prior to the treatment, and housing returns in the quarter before the treatment.[27] Predictors also include median income, unemployment rate, and population density in 2007. We estimate each Synthetic counterfactual at the highest frequency data available. For Zillow and MDRI data this is the monthly frequency, while we use quarterly time series for MBA foreclosure starts and FHFA housing returns. All available states in the contiguous US constitute the set of potential controls (the donor pool).

The results are in tables 3 and 4 and in figure 2. To start, table 3 displays the contribution of each state to California's Synthetic Control for each outcome variable. Here, we list the SCM weight applied to each state. For brevity, only states with a positive weight are listed. The results generally match our expectations in that California is paired with other housing boom-bust states for all outcome variables. In panel A for foreclosure starts, REO foreclosures, or the MDRI, California's Synthetic is comprised largely of Nevada, Arizona, Florida, and Maryland, all states that experienced substantial house price busts during the recent crisis. Results in panel B similarly show that Nevada and Florida largely constitute California's Synthetic for both FHFA and Zillow housing returns. For example, California's Synthetic for the Zillow All Homes returns is built largely from Nevada and Florida (two-thirds weight) with additional weight applied to Rhode Island. Nevada, Florida, Minnesota, and Rhode Island also all receive substantial weight in the construction of the Synthetic counterfactuals for California Bottom Tier and Top Tier returns. In total, these matches are congruent with our expectations as California is best approximated by other housing bust states.

Graphically, the accuracy of the Synthetic matches for the pre-treatment period is seen in figure 2 (left of long-dashed-red vertical). Here, for each outcome variable we plot the path of California and its Synthetic counterfactual versus the sample average. The black line is California, the blue line is its Synthetic Control, and the gray line is the sample average. The vertical lines are the same events highlighted in figure 1. As seen across the plots, during the pre-treatment period the path of the California and the Synthetic move in lockstep with correlations that are all approximately 1, while the pre-treatment sample average deviates notably from California in every plot. The Synthetic Control hence creates suitable matches for California. For example in panel 2A, REO foreclosures in California and its Synthetic counterpart barrel

[27]We also include the outcome variable as an additional predictor in cases where the outcome variable is not housing returns.

upwards simultaneously through 2008 as the crisis permeated through housing markets. In marked contrast, the rise in REO foreclosures for the sample average was relatively muted. The dynamics across California and the Synthetic versus the sample average reveal the geographic heterogeneity of the crisis and how the data-driven SCM adroitly constructs a counterfactual. The other plots in figure 2 similarly highlight a close co-movement between California and its Synthetic during the pre-treatment period.

The Synthetic Control estimation results are in table 4. In table 4, for each variable we show the pre-treatment root mean-squared forecast error (RMSFE) and the change in the path of the outcome variable for the period 2008Q3 - 2010Q4, from the passage of SB-1137 through the sunset date for the CFPA, for both California and its Synthetic Control. House prices and the MDRI are presented as the change in logs over the treatment period; all other variables are the cumulative sum of the levels. The Gap between California and its Synthetic in column (4) is the estimated treatment effect. We also conduct a permutation test where the treatment is iteratively applied to all available control units; this process yields a Gap estimate in each of these placebo experiments. The percentile of the Gap for California, relative to all of the estimated placebo effects, the Gap Percentile, is column (5). Asterisks in the table indicate instances where the Gap for California is in the upper (lower) 85, 90, and 95th (5, 10, and 15th) percentiles relative to all estimated placebo effects.

First, the pre-treatment RMSFEs between California and its Synthetic Control, in column (1) of table 4, are all small in magnitude and show that that the Synthetic closely tracks California for all outcome variables over the pre-treatment period. Indeed, the pre-treatment RMSFEs are less than one-tenth of the pre-treatment annualized standard deviations. Panel A in table 4 presents the SCM results for foreclosure starts, REO foreclosures, and the MDRI. During the CFPL period, 15.96 percent of California mortgages entered foreclosure (foreclosure start), compared to 25.34 percent for the Synthetic Control. The Gap between these estimates, the treatment effect from the CFPLs, is −9.38. Hence, 9.38 percent fewer California mortgage loans entered default from 2008Q3-2010Q4, implying that the CFPLs lowered the portion of homes that entered foreclosure by an economically meaningful 37 percent. The magnitude of the Gap estimate is similar for prime foreclosure starts, but greatly magnified for subprime foreclosure starts. Yet compared to the portion of subprime loans that entered into default for the Synthetic, the CFPLs also lowered subprime foreclosures by 30 percent (20.61/66.32).

Graphically, the causal impact of the CFPLs on foreclosure starts is displayed in 1A - 1C of figure 2. After the introduction of the CFPLs, foreclosure starts fell markedly compared to the Synthetic, indicating that CFPLs dramatically reduced the incidence of default in California.

Zillow REO foreclosures and the MDRI document a similar amelioration of the housing crisis due to the introduction of the CFPLs. Relative to the Synthetic counterfactual, table 4 panel A shows that REO foreclosures per 10,000 people fell by 307 (32 percent) and household Mortgage Default Risk fell by half. Column (5) shows the Gap estimates for foreclosures and the MDRI are all in the 0th percentile relative to all placebo effects, indicating the effects of the CFPL treatment effect were rare and large in magnitude. Panels 2A and 2B of figure 2 further display the notable drop in REO foreclosures and Mortgage Default Risk following the introduction of the CFPLs as these key indicators fell immediately following the policy intervention and remained low through the end of the sample period.

In panel B of table 4, we show the estimation output when FHFA and Zillow housing returns are the outcome variables. During the CFPL period, California FHFA house prices fell 20.19 percent, while those for the Synthetic plunged 40.06 percent. The corresponding Gap and thus the estimated treatment effect of the CFPLs for FHFA house price returns is 20 percent. Likewise, California Zillow All Homes house prices slipped 22.03 percent as prices for the Synthetic fell 32.25 percent, yielding a Gap estimate of 10.22 percent. This latter effect, the more conservative of our state-level estimates, is large in magnitude and implies that the CFPLs reduced the fall California house prices from 2008Q3-2010Q4 by one-third. Further, the Gap Percentiles of the estimated treatment effect for both the FHFA and Zillow All Homes indices, relative to all placebo effects, are 100, supporting a causal interpretation of the results. Graphically, panels 2C and 3A of figure 2 show that following the implementation of the CFPLs that California housing returns increased notably. For case of the FHFA returns in panel 2C, housing returns jumped from nearly -10 percent per quarter just prior to the treatment to 0 percent by late 2009. In contrast, housing returns for the Synthetic were negative throughout most of the treatment period.

Panel B of table 4 also shows that the CFPL relative house price gains extended to all housing market tiers, but were largest for Bottom Tier Homes. Moreover, the Gap Percentiles across the Zillow housing market tiers are all near 100 and therefore support a causal interpretation of the results. Altogether, these state-level results show that the CFPLs reduced the slide in California housing markets and attenuated the negative effects of the 2000s housing crisis.

5.1.1 Robustness of the State-Level Synthetic Control Results

A potential concern with the above analysis is that the estimates produced by the SCM may hinge on the inclusion of a particular state in the construction of the Synthetic counterfactual. To address this issue, we iteratively eliminate each state as a potential control and retain all other states. Then for each of these iterations we build a new Synthetic Control and record the corresponding CFPL gap estimates. In table D1 of appendix D, we report the minimum absolute Gap estimate (the Gap estimate that is closest to zero) for each of the foregoing variables. As is evident, results are comparable to those described above, implying that our findings are robust to alternative control groups and samples.

5.2 County and Zip Code Synthetic Control Results

Next, we implement our Synthetic Control approach at the county and zip code levels. The housing data available at the county level include Zillow REO foreclosures and house prices across housing market tiers. At the zip code level, we only have access to median house price indices. Both the county and zip code house data are monthly, and we search for a Synthetic counterfactual using the same predictor set outlined above.[28] The results are in table 5. Column (1) shows the number of available California regions for each outcome variable; the mean weighted pre-treatment RMSFE is in column (2); column (3) reports the mean weighted CFPL Synthetic Control Gap estimates; the standard errors of the mean Gap estimates are in column (4); and column (5) displays the percentage of households living in a county or zip code with a Gap Percentile greater than 85. For REO foreclosures, column (5) reports 100 minus the gap percentile. In columns (2) and (3) the number of households are used as weights, and the significance of the mean Gap estimates is assessed using the standard errors in column (4).

Overall, the results in table 5 are congruent with our above state level estimates as we find

[28]For each outcome variable the predictor set includes housing returns, pre-treatment house price growth and housing return variance, house price growth in the year prior to the treatment, housing returns in the quarter before the treatment, median household income in 2007, the unemployment rate in 2007, and population density in 2007. For the foreclosure data, we also use average monthly foreclosures as a predictor variable. County-level unemployment rates are mapped to the zip code level using the Missouri Data Bridge. To ease the computational burden of the Synthetic Control optimization routine in the zip code analysis, we restrict the donor pool to zip codes in Arizona, Florida, and Nevada. This leaves 1128 zip codes that can be used to build a Synthetic Counterfactual for each California zip code. Furthermore, at the zip code level we also select V in equation 3 for all zip codes by randomly selecting 50 California zip codes and using the median variable weights. No restrictions are placed on the county-level donor pool or Synthetic Control estimation procedure.

a marked improvement in California housing markets due to the CFPLs. To start, panel A presents the SCM estimation results for county-level REO foreclosures per 10,000 people. Column (1) reports that the Zillow REO foreclosure data are available for 21 California counties, and column (2) shows that for these 21 counties that the average pre-treatment RMSFE was just 1.29. Compared to the average pre-treatment annualized standard deviation of 28.10, the average pre-treatment RMSFE is small in magnitude. Thus, the SCM constructs apt counterfactuals for California counties when REO foreclosures are the outcome variable. Column (3) shows that the CFPLs lowered California foreclosures by 120 per 10,000 people. This estimate is significant, large in magnitude and economically meaningful. Indeed, using the 2007 population estimates, these results imply that the CFPLs prevented 380,000 REO foreclosures, a reduction in REO foreclosures of 16 percent.[29] Furthermore column (5) of panel A shows that a full two-thirds of California households lived in counties where the Gap estimate was in the 85th percentile relative to all placebo effects, implying that the reduction in REO foreclosures was large relative to placebo effects and spread to many households.

Panel B of table 5 presents the SCM estimation output for zip code level house prices. As seen in column (1), the results include nearly 1200 California zip codes. At this level of disaggregation, the Synthetic Control can exploit zip code level housing and macro variables in the predictor set and generate highly localized CFPL estimates. Thus, the zip code results in panel B are our preferred CFPL house price estimates. Column (3) of panel B shows that the CFPLs led to an 9.6 percent increase in California housing prices, yielding an increase in housing wealth of $450 billion. In many California zip codes, these effects were rare and large in magnitude relative to placebo estimates as noted in column (5) which shows that over 50 percent of California households lived in zip codes with a Gap Percentile greater than 85.

A potential concern with the use of the highly disaggregated zip code data is that the results might be driven by pre-treatment matching errors between California zip codes and their Synthetic counterparts. This concern may arise even if the mean weighted pre-treatment RMSFE is small as in column (1) of table 5.[30] We investigate this issue in figure 3. Here for each zip code we plot the CFPL house price growth Gap estimates versus the pre-treatment RMSFEs. Note that the horizontal axis is in logs and that the points and the regression line

[29]The 2007 population for the 21 counties in the SCM REO foreclosure analysis have a population of 31,616,514. Thus 31,616,514·(119.89/10000) = 379050.4

[30]For example, if the high RMSFE zip codes were extreme outliers in terms of the CFPL house price growth estimates.

are weighted by the number of households. Clearly with an R^2 of approximately zero, the pre-treatment RMSFEs explain nearly none of the variation in the Gap estimates. Moreover, the slope estimate is negative (but not significant), suggesting that, if anything, Gap estimates fall as matching errors increase.

Next, panel C of table 5 displays the county-level CFPL house price growth estimates. Column (2) shows that the pre-treatment RMSFEs are small in magnitude and thus that the SCM on average builds suitable counterfactuals for California counties. The mean Gap estimates are in column (3) and are all positive and statistically different from zero at the one percent level. The results for the Zillow All Homes indices indicate that the CFPLs led to a 6.18 percent increase in California house prices, the most conservative CFPL house price growth estimate in this paper.[31] Using this estimate, we find that the CFPLs created \$312 billion dollars in housing wealth.[32] The next two rows in panel C show that the CFPL house price gains were nearly 10 percent for the Bottom Tier homes, but just 2.85 percent for Top Tier homes. Finally, column (5) of panel C implies that over 42 percent of Californians lived in counties with a Gap Percentile greater than 85 using the Zillow All Homes index. These numbers for the Bottom and Top Tier indices are 42 and 30 percent, respectively.

As noted in the introduction, foreclosure reduction is the key channel through which the CFPLs can generate house price gains. Thus, if the aforementioned house price growth is attributable to the CFPLs, then we should see a negative relationship between the CFPL Gap in foreclosures and the CFPL gap in house price growth. That is, fewer foreclosures translates into higher house price growth. We examine this relationship at the county level in figure 4. Here for the 21 California counties with available data, we plot the CFPL Gap in house price growth versus the CFPL Gap in REO foreclosures. In panel 1A, we plot the CFPL Gap in All Homes house price growth versus the CFPL Gap in REO foreclosures per 10,000 people. The plot clearly shows a strong negative relationship between the CFPL Gaps in REO foreclosures and house price growth as the regression R^2 of 0.58 indicates the REO foreclosure Gap explains nearly 60 percent of the variation in the All Homes house price Gap. Thus, the foregoing CFPL house price gains are largely attributable to the CFPL drop in foreclosures. The slope coefficient of -0.03, which is statistically significant at the one percent level, suggests that 100 fewer foreclosures per 10,000 people leads to a 3 percent increase in house prices. Panel

[31]For median or all homes estimates. Our estimates for Top Tier homes are smaller.

[32]The total number of California housing units from the 2008 1-Year ACS Community Survey Table S1101 is 12,176,760. \$413,200*(0.0618)*12,214,891 $\approx$ \$311.9 billion.

1B shows that the effect of foreclosures on house prices is twice as large for Bottom Tier homes, while 2A finds a similar effect for homes in the Top Tier. The Bottom Tier results are notable as they show the outsized CFPL effects on the lower end of the housing market. In both panels 1B and 2A, the slope coefficient is negative and significant at the 1 percent level and the R^2 statistics are large in magnitude. The plots in figure 4 also provide causal estimates of the impact of foreclosures on house prices as the Gaps in REO foreclosures and house prices are derived from an exogenous policy shock. These results thus contribute to a recent literature that aims to estimate the causal effects of foreclosures on house prices during a crisis.[33]

We report the geographic salience of the zip code level CFPL effects across key California regions in table 6. The region definitions are in the notes to table 6. For each region, the table displays the CFPL Gap mean weighted house price growth, the percentage of zip codes in the region with a Gap Percentile greater than 85, and the number of zip codes. The areas with the largest house price gains are the Inland Empire (column (2); Inland Southern California), Los Angeles (column (3)), and Southern Los Angeles (column (6)). In the Inland Empire for example, house prices increased 12.48 percent due to the CFPLs and 58 percent of zip codes had Gap percentile greater than 85. These results for the Inland Empire are important as they show that the beneficial CFPL effects extended to one of the hardest-hit, lower income geographic areas of California and are congruent with our above findings that indicate that the CFPLs had a positive effect on the lower end of the housing market. The CFPL house price gains were also large in Los Angeles and Southern Los Angeles with Gap estimates above 16 percent.

At the county level, the choropleth plots in figure 5 show the geographic distribution of the CFPL REO foreclosure and house price effects. Darker colors correspond to larger effects, while the counties in white have no available data. County names are printed on the plots if the Gap Percentile is greater than 85 and one, two, or three asterisks represent a Gap Percentile that is greater than 85, 90, or 95 (for foreclosures we report 100 minus the Gap Percentile). Panel 1A presents the Gap in REO Foreclosures across counties. The effects are largely concentrated in Southern and Central California as there are large reductions REO foreclosures in Los Angeles, San Bernardino, Kern, Ventura, Tulare, and Fresno counties. We find little alleviation of foreclosures in Northern California. There is also an outlying county in Northern California (dark blue in the map), Stanislaus county. Yet the pre-treatment RMSFE

[33] See, for example, Anenberg and Kung (2014), Gerardi et al. (2015), and Mian et al. (2015).

for Stanislaus was 8 times the sample median, indicating that the SCM did not find a suitable counterfactual for Stanislaus. Panels 1B, 2A, and 2B show the CFPL Gaps in house price growth across counties. Again, the CFPL effects are largely concentrated in Southern and Central California. Moreover, the Bottom Tier homes in panel 2A show that the beneficial CFPL effects permeated across Southern California.

5.2.1 Non-Judicial Foreclosure States

The previous SCM estimates used counties or zip codes in both judicial *and* non-judicial foreclosure states in the donor pool. Yet judicial and non-judicial foreclosure states differed markedly during the crisis in the duration and costs of the foreclosure process and the subsequent effects of foreclosures.[34] Further as noted above, California began the crisis as a non-judicial foreclosure state and the aim of the CFPLs was to increase the time and pecuniary costs of the foreclosure process to encourage mortgage modification. Hence, the CFPLs transformed California's housing laws to increasingly mimic those in judicial foreclosure states. Our above analysis that uses both judicial and non-judicial states is thus conservative in nature.

We re-estimate the CFPL effects, but only use *non-judicial* foreclosure states in the SCM donor pool. The results are in table 7, where the layout of table 7 is identical to table 6. Notice first that the pre-treatment RMSFEs (column (2)) remain consistently small in magnitude and thus the SCM builds apt Synthetic counterfactuals based on this subset of the data. Overall, the results match our expectations and show that the CFPLs effects are larger when the donor pool consists of only non-judicial states. For example, the weighted mean drop in REO foreclosures per 10,000 people due to the CFPLs (panel A, column (3)) is 139.29. Our above estimate that used both non-judicial and judicial states in the donor pool was 120.16. Thus, when using only non-judicial foreclosures states, our estimate for the decline in REO foreclosures attributable to the CFPLs increases by 16 percent. Moreover, the number of households that lived in a county with a Gap Percentile for REO foreclosures greater than 85 increases 10 percentage points to 76 percent. Table 7 also documents an uptick in the Zillow zip code, All Homes county, and Top Tier county house price growth.[35]

[34] See Mian et al. (2015) and Gerardi et al. (2013).

[35] Bottom Tier house price growth is the only variable that does not increase in magnitude, but instead falls by a little over 2 percentage points.

6 Were the CFPL Foreclosure and House Price Effects Transitory?

During the crisis, leading federal policymakers advocated against policy interventions similar to the CFPLs, suggesting that such policies would simply prolong the crisis but not materially improve housing market outcomes.[36] This line of thinking thus implies (1) that effects of the CFPLs should reverse after the conclusion of the policy once the CFPL restrictions were lifted; and (2) that the number of homes lingering in foreclosure or late-stage delinquency should rise as mortgage lenders wait to foreclose on these properties. We assess these hypotheses by re-examining figure 2 and through figure F1 of appendix F. First, if the CFPL policy effects reversed, foreclosure starts and REO foreclosures should spike after the conclusion of the policy in panels 1A through 2A of figure 2. We see no such reversal as neither foreclosure starts nor REO foreclosures rise after the conclusion of the policy period. Instead, these variables remain below their Synthetic counterfactuals through the end of 2014, suggesting that the CFPL policy effects were long-lasting. Figure F1 in appendix F further considers the above concerns as put forth by federal policymakers. In particular the figure plots the state-level SCM results for 60 and 90 day delinquencies, serious delinquencies (in excess of 90 day delinquencies), and the foreclosure inventory (loans at some point in the foreclosure process). None of these variables rise with the implementation of the CFPLs. Rather, seriously delinquent loans and the foreclosure inventory fall, implying that in the wake of the conclusion of the CFPL intervention there was not an increase in the number of homeowners lingering in persistent late stage delinquency or foreclosure. Hence, the CFPLs did not prolong the crisis.

7 Did the CFPLs Increase Mortgage Modifications?

We also investigate the CFPLs' impact on mortgage modification as that was the overarching aim of the policy intervention. In doing so, we build on recent mortgage modification literature and use a diff-diff framework.[37]

Our data comes from the Fannie Mae Loan Performance Dataset. The data are a representative subset of Fannie's GSE conforming loan portfolio with acquisitions dating back to 2000. Importantly, the data follow each loan monthly and report delinquency status, a flag for modification, and other variables including original loan characteristics.

[36] "Lawrence Summers on 'House of Debt' ". *Financial Times*. June 6, 2014. "Geithner Calls Foreclosure Moratorium 'Very Damaging' ". *Bloomberg News*. October 10, 2010.

[37] See for example, Mayer et al. (2014), Agarwal et al. (2015), and Agarwal et al. (2017).

We start by restricting our dataset to avoid other mortgage modification programs as potential confounds. First, we limit the CFPL treatment period to 2008M07 - 2009M02, prior to the announcement of HAMP and HARP, the federal crisis-period mortgage modification and refinancing programs. Second, by using only the conforming, conventional loans from the Fannie dataset, we eliminate any possible contamination related to the Countrywide subprime settlement and subsequent modification program.[38] Next since the goal of the CFPLs was to aid borrowers facing default, we consider a repeated cross-section of delinquent loans. For the treatment period ranging from July 2008 to February 2009, the dataset only includes loans that were 30, 60, or 90 days delinquent at any point in 2008Q2, prior to the announcement of the CFPLs. By subsetting the data based on delinquency status in the quarter prior to the announcement of the policy, we eliminate any possible contamination between delinquency status and program treatment. The pre-treatment period is a one year lag of the CFPL treatment period, from 2007M07 - 2008M02 and includes all loans that were 30, 60, or 90 days delinquent at any point in 2007Q2. Using a one year lag for the pre-treatment period circumvents any seasonality concerns regarding delinquencies, modifications, or other housing market dynamics. Finally, to build comparable control and treatment groups, we consider loans from Arizona, California, and Nevada. As noted above, the housing market dynamics were highly similar across these three Sand States. These states are also all non-judicial foreclosure states and thus their foreclosure processes were comparatively similar before the implementation of the CFPLs.

Table E1 in appendix E compares borrower quality at origination across the control (AZ, NV) and treatment (CA) groups for both the pre-CFPL and the CFPL treatment periods. In particular, we report the mean and standard deviation of the FICO credit score, the debt-to-income ratio, and the interest rate at origination. Clearly, borrowers across the treatment and control groups are of similar quality in both the treatment and pre-treatment periods: Their average credit scores are nearly equivalent, their mean debt-income-ratios are similar, and their average interest rates are both a little above 6 percent. The standard deviations across the two groups are also comparable.

[38] Countrywide in October 2008 entered into a multi-state settlement where it agreed to modify subprime first-mortgage loans. See Mayer et al. (2014) for more details.

Using the diff-diff setup, our econometric specification employs a probit model as follows:[39]

$$\Pr(Y_{it} = 1 \mid \text{Delinquent}) = \Phi(\mathit{California}_{it}\beta + \mathit{CFPL}_{it}\mu + \mathit{California}_{it}\mathit{CFPL}_{it}\delta + \mathbf{x}'_{it}\gamma) \quad (4)$$

where Y_{it} equals one if a loan was modified and zero otherwise. Note that we treat modification as an absorbing state and thus if a loan was modified in the pre-treatment period it is not used in the subsequent treatment period. Delinquency is defined as above. $\mathit{California}_{it}$ equals one if the loan is associated with a home in California and CFPL_{it} takes a value of one for CFPL treatment and zero otherwise. $\mathbf{x}_{it}$ is vector loan and borrower characteristics used as controls including a factor variable for delinquency status at the start of the pre-CFPL or CFPL periods[40] and a factor variable for the year of origination. The control set also includes the following variables at origination: the interest rate, FICO credit score, debt-to-income ratio, and log of the loan amount. With regard to house prices, $\mathbf{x}_{it}$ includes the three digit zip code level log of the house price in 2007Q1, house price growth in 2007, and housing return variance from 2004Q1 - 2008Q2. Last, we account for macro factors through the county unemployment rate and the three digit zip code income per household in 2007.

The key coefficient of interest, δ, tracks the difference-in-differences across the treatment and control groups – the estimated change between California and the control group across the pre-CFPL and CFPL periods.

Table 8 displays the results. Column (1) estimates the regression in equation 4 without any controls, while column (2) accounts for loan-level borrower characteristics, column (3) uses both loan-level and house price controls, and column (4) employs a full battery of controls that span loan-level characteristics, housing market variables, and macro indicators. The coefficients in the table are the marginal effects from the probit model and heteroskedasticity robust standard errors are in parentheses. The bottom row of the table reports average modification rate in pre-treatment period ("Prob(Modify | Delin, Pre-CFPL)").

First, in all regressions the coefficient on *California* is small in magnitude and is only marginally significant at the 10 percent level in columns (1)-(3). Yet when we include a full battery of controls in column (4), the coefficient on California becomes insignificant. This suggests that the probability of modification across the treatment and control groups was not statistically different in pre-CFPL period. The parameter of interest is the *California* $\times$ *CFPL*

[39]For the estimated treatment effects in non-linear probit models see Puhani (2012), Ai and Norton (2003). See also Mayer et al. (2014).

[40]Fannie tracks delinquency status at 30 day intervals

interaction. In all of the models in table 8, the *California* × *CFPL* is positive, statistically significant, and economically meaningful. Using the full model in column (4), the probability of modification increased 0.5 percentage points for California, compared to the control group, in the CFPL period. Relative to the average pre-treatment modification rate listed in the last row of the table (1.7 percent), this diff-diff estimate corresponds to a 29 percent relative increase in the modification rate for California during the CFPL period. Hence, modifications increased markedly in California with the introduction of the CFPLs.

8 The CFPLs, HAMP, and HARP

In the wake of the late-2000s crisis, the federal government in March 2009 announced HAMP and HARP programs. For our purposes, a particular concern is that these programs represent a potential confound contaminating our above CFPL estimates. This is unlikely. First, the CFPL policy effects materialized before the announcements of HAMP and HARP. A review of panels 1A-2A of both figures 1 and 2 clearly shows that foreclosure starts and REO foreclosures fell in California prior to the announcements of HAMP and HARP in 2009M03. Indeed, using our county-level Zillow data we find that the CFPLs reduced foreclosures prior to March 2009 by 24.38 per 10,000 people, 20 percent of the overall treatment effect in table 5. Further, the time period before the announcement of HAMP/HARP constituted 23 percent of the overall CFPL treatment period. Thus, extrapolating the pre-HAMP/HARP CFPL treatment effect to the entire CFPL treatment effect would yield an estimate very similar to that reported in table 5. This, combined with figures 1 and 2 which show large reductions in foreclosure starts and REO foreclosures immediately following the CFPL policy announcement, strongly support the contention that the CFPL treatment effects were *not* generated by HAMP or HARP. Similarly, in section 7 we show that the CFPLs led to higher modifications prior to the announcements of the federal mortgage modification programs.

Further as discussed in Agarwal et al. (2015), it is important to note that the implementation of HAMP and HARP was substantially delayed beyond their enactment date. HARP, for example, did not begin in earnest until a year after the policy announcement in March 2010. By this point California housing markets had improved dramatically compared to their Synthetic counterparts. Larry Summers, the Director of the National Economic Council during the crisis, further echoes this point saying that in 2009 among federal policymakers that "...there was intense frustration with how few homeowners our programmes were reaching..."[41]

Finally, HAMP and HARP were national in scope and thus these programs would only contaminate our CFPL estimates if they differentially affected California relative to the control group. Instead, evidence suggests that regional characteristics do not explain HAMP effects and that differential HAMP effects were based on pre-HAMP factors.[42] On the basis of these factors, we conclude that HAMP did not change housing market trends in California relative to controls nor did it confound our estimates of the CFPL effects.

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

The passage of the CFPLs increased the cost of the foreclosure process for lenders and thus ex post, may have reduced the value of their foreclosure option on originated loans. As noted by Alston (1984) in his analysis of foreclosure moratoria during the Great Depression, if the value of the foreclosure option declines, lenders may respond by either (1) increasing the interest rate on new mortgages to compensate for the reduced value of the foreclosure option; or (2) rationing credit, especially in environments where raising interest rates is infeasible, and only lending to higher quality borrowers. For the CFPLs, (1) would translate into fewer loans being originated in California in equilibrium in the wake of the policy implementation, *ceteris paribus*. With regard to (2), Alston notes that during the Depression era, lenders may have been reluctant to increase interest rates as this would have created "hostility and ill will" (p. 451). Similar concerns, along with heightened government scrutiny, may have also deterred lenders from increasing interest rates in California following the 2000s housing crisis.

Conversely, in their report on the CFPA, California (2010) notes that the number of applications for an exemption from the CFPA foreclosure moratorium was lower than anticipated, suggesting that the lender value of the foreclosure option was limited given the depths of the crisis. In the context of the severe economic and housing market downturn, the CFPLs may not have not altered banks' expectation of the value of the foreclosure option post-policy implementation. Finally, if the CFPLs aided depressed California housing markets (as documented above), then lenders may have viewed the CFPLs favorably as excess foreclosures create dead weight losses for lenders (Bolton and Rosenthal, 2002) and higher house prices increase the

[41] "Lawrence Summers on 'House of Debt' ". *Financial Times*. June 6, 2014.

[42] Specifically, Agarawal et al. 2017 find that the low take-up rate of HAMP modifications was due to pre-HAMP differences in modification rates across services. Agarawal et al. 2017 also note that differences are not due to regional characteristics of mortgages. See p. 25 of the *NBER* 2012 working paper version.

value of repossessed homes.[43]

We employ the HMDA dataset to determine the impact of the CFPLs on home purchases following the implementation of the policy. We consider only consider loans not sold to GSEs as GSEs do not discriminate based on a borrower location at the state level (Hurst et al., 2016). The inclusion of loans sold to GSEs does not change our results. The results are in table 9. First, we use loan-level data to determine whether the probability of being denied a mortgage is higher in California, in line with a credit rationing response for new borrowers following the CFPLs. Specifically, we consider a probit model where the left-hand-side variable is an indicator that takes a value of one if the prospective borrower was denied a mortgage and zero otherwise.[44] The key right-hand-side variable is an indicator that takes a value of one if the home is in California and zero otherwise. Controls include the log of the loan amount and log of applicant income; the Zillow All Homes house price return and the growth in IRS income and IRS population the year before the loan application was submitted for the home's zip code; and indicator variables for applicant race and applicant sex. These data range from 2009 to 2014. We first restrict the dataset to Arizona, California, Florida, and Nevada (9 column (1)), as the housing dynamics of these states were similar prior to the implementation of the policy during the 2000s; yet for robustness we also consider a dataset with California, Colorado, New York, and Texas (column (2)), as these latter states that were less affected by and rebounded relatively quickly from the crisis. Columns (1) and (2) report the marginal effects from a probit model. Heteroskedasticity robust standard errors are in parentheses. A positive coefficient on the indicator for California would suggest that Californians were more likely to be denied mortgage credit, all else equal, and the CFPLs had an adverse on new California borrowers. If anything, the results in columns (1) and (2) show opposite: The probability of denial in the post-treatment period was slightly lower for California (though the coefficient on California in column (1) is insignificant). Hence, Californians were no more likely than residents in the other states to be denied mortgage credit in the wake of the CFPLs.

Next, in columns (3) - (6) we consider loan volume growth following the implementation of the policy. Specifically, we consider loan growth at the zip code level, both in terms of the number and dollar volume of loans, for 2009 through 2014 relative to 2007 using only loans

[43]Along these lines, Bolton and Rosenthal (2002) develop a theoretical framework and show that moratoria always increase efficiency ex post, following an adverse shock.

[44]If a mortgage application was denied we do not know if was eventually going to be sold to a GSE.

not sold to GSEs.[45] The key right-hand-side variable is an indicator that takes a value of 1 for California and controls include applicant income growth; IRS zip code level income and population growth and Zillow house price growth for 2008-2009 (crisis), 2010-2011 (emergence from crisis), and 2012-2014 (post-crisis). Here, if mortgage lenders were rationing credit to California zip codes, relative to those in other states, the coefficient on California would be negative. Again, we find the opposite effect. The estimates in columns (3) - (6) imply that loan volume growth, in terms of both dollars and the number of loans originated, was instead higher in California zip codes. In total, the results in table 9 show that new California borrowers were not adversely affected by the CFPLs.

10 The Impact of the CFPLs on the Real Economy – Auto Sales

Several recent papers have used new auto sales to assess the impact of housing market changes on durable consumption and hence the real economy.[46] We similarly adopt this approach using county-level new auto sales registrations from RL Polk. In addition to the quantity of auto sales registrations, we also compute dollar expenditures within each county as in Mian et al. (2013).[47] Mian et al. (2013) find that marginal propensity to consume (MPC) out of changes in housing wealth is largest for auto sales followed by other durable goods. We would thus expect any real economic effects of the CFPLs to be visible in autos.

We first use the quantity of new auto registrations within the SCM framework. Our aim is to determine the change in auto sales due to the CFPLs by relating California counties to their counterfactuals. Specifically and in line with the above approach, for each California county the SCM constructs a Synthetic Control based on the following housing and macro variables: housing returns, house price growth during the pre-treatment period (2004Q1-2008Q2), pre-treatment housing return variance, house price growth one year prior to the treatment, and the median income, unemployment rate, and population density in 2007. In addition, the predictor set also includes the log quarterly auto sales relative to the log value in 2008Q2, the growth in auto sales during the pre-treatment period, and the growth in quarterly auto sales one year prior to the treatment. The weights on each of these are chosen to minimize the pre-treatment

[45] Specifically, we define the loan volume growth as $(\ln(\text{Loan\_vol}_{2009}+\cdots+\text{Loan\_vol}_{2014}))-(\ln(\text{Loan\_vol}_{2007}))$.

[46] See, for example, Mian et al. (2013), Mian et al. (2015), Agarwal et al. (2015), and Agarwal et al. (2017).

[47] Specifically, for each year we allocate the census retail expenditures on new autos to each county based on the portion of new auto registrations in the RL Polk data. Mian et al. (2013) note that this procedure introduces measurement error as information on prices is not available. However, any potential measurement error would be nullified if prices change equally in all counties.

RMSFE between the log of quarterly auto sales and the log value in 2008Q2. Note that Mian et al. (2015) find that auto sales during the crisis differed sharply across judicial and non-judicial states. Thus not surprisingly, our also results differ based on the control set. If we use all available counties, across both judicial and non-judicial states, the results suggest that the weighted mean of CFPL auto sales growth across California counties was -6.07 percent (White t-statistic = -2.19). Yet as noted above, California is a non-judicial foreclosure state and the aim of the CFPLs was to increase the time and the pecuniary costs of the foreclosure process. Thus, a more appropriate control group consists of *only non-judicial* foreclosure states. Using the non-judicial control group, California CFPL mean weighted auto sales increased 12.46 percent (White t-statistic = 9.34). Hence, relative to a control group comprised of counties in non-judicial states, California auto sales increased notably in the CFPL period.[48]

The top panel in figure 6 plots the path of auto sales in Los Angeles county, California's largest county, versus its Synthetic Control where the donor pool consists of only non-judicial foreclosure states. Auto sales are in log deviations from 2008Q2. During the pre-treatment period, auto sales in LA county and its Synthetic are highly correlated and fall at the onset of the crisis. Then after the implementation of the CFPLs, the fall in LA county auto sales is mitigated in some quarters. Towards the end of the policy period and into 2012, LA county auto sales are noticeably higher in nearly every quarter. The performance of LA auto sales later in the sample period is not surprising as changes in housing wealth translate to consumption with a lag (Carroll (2004) and Mian et al. (2013)). The bottom panel of figure 6 further highlights the difference in auto sales between LA county and its Synthetic. Here for each period we plot the percentage point difference in total auto sales growth relative to 2008Q2 between LA county and its Synthetic staring in 2008Q2. The differences between LA and its Synthetic are stark: At the end of the policy period in 2010Q4, total auto sales growth relative to 2008Q2 was 14.3 percentage points higher for LA county relative to the Synthetic counterfactual. By 2013Q1, this number had grown to 50 percentage points. Clearly, following the house price growth generated by the CFPLs, auto sales growth increased markedly in LA county.

Next, we assess the impact of CFPL house price growth on auto sales growth within California. Our point of departure is the first key estimating equation from Mian et al. (2013):

$$\Delta \log C_t^i = \alpha_t + \beta \cdot \Delta \log X_t^i + \varepsilon_t^i \tag{5}$$

[48]For both the non-judicial only and all states donor pools, the pre-treatment RMSFEs are small in magnitude and thus the SCM can construct suitable counterfactuals for California counties.

where $\Delta \log C_t^i$ is the natural log change in consumption for household i and $\log X_t^i$ is the natural log change in housing wealth.[49] The parameter of interest, β, measures the elasticity of consumption with respect to housing wealth and the null hypothesis is that households are completely hedged against future changes in housing wealth, $H_0 : \beta = 0$. While Mian et al. (2013) exploit an instrumental variable approach to generate causal estimates for 5, we simply use the CFPL house price growth treatment effects that were the result of an exogenous policy shock to produce causal estimates. Further, our setup is also advantageous and may be informative for policymakers as we estimate the response of consumption to a positive, policy induced housing shock during a crisis. We are aware of no work that capitalizes on a comparable framework. Other recent studies instead use cross-sectional variation in negative housing market shocks during the crisis. Thus importantly, if our consumption results differ from related papers, it may suggest that estimates of the response of consumption to changes in housing wealth may be uninformative for policymakers.

The results are in table 10. Note that these estimates employ county-level data and thus use only 39 observations. Column (1) estimates equation 5 with no controls and shows that a one percent increase in CFPL house price growth leads to a significantly significant 0.285 percent increase in auto sales growth, an estimate in line with the previous literature. Hence, CFPL house price growth translates into real economic effects. Column (2) indicates that the effects are non-linear and thus that effects may be heterogeneous across CFPL house price increases (Mian et al., 2013). In column (3), we show that the results are robust to the inclusion of pre-treatment housing proxies as additional controls. Finally, the model column (4) employs both housing and macro controls. The estimate falls to 0.206. In total, table 10 indicates that CFPL increases in house price growth lead to increases in auto sales growth.

Last, we estimate the average MPC associated with the CFPL-induced house price shocks. Again, the CFPL house price growth gap estimates serve as a proxy for the change in the growth housing wealth. To compute the average MPC we convert all variables to dollar changes. Dollar changes in auto sales are constructed following the procedure outlined above, and dollar changes in home values are calculated by multiplying the CFPL house price growth by the Zillow median house price estimate within each zip code. The results are in table 11. Column (1) regresses the CFPL Gap in the change in auto spending on the CFPL Gap in the change in home values. Both variables are in changes in thousands of dollars. The average MPC

[49] See Mian et al. (2013) and the references therein for the derivation of equation 5.

estimate is 0.6 cents and statistically significant at the one percent level. This estimate is in line with the literature.[50] Column (2) shows that there might be some non-linearity in the MPC out of housing wealth, but the estimate on the squared CFPL housing wealth changes is not statistically significant. Columns (3) and (4) show that our average MPC estimate is stable and significant even after the inclusion of additional controls.

10.1 The CFPLs and Mortgage Refinancing Volume

Access to credit is a key potential channel through which CFPL house price growth can lead to increases in durable consumption and related real-side economic effects. Table 12 investigates this channel and shows regressions of the growth in HMDA refinancing volume on (1) an indicator for California using a Sand States sample; and (2) the CFPL Gap in house price growth within California. Both regressions are at the zip code level. Controls include proxies for income, population, and house price growth. The results are notable: Following the CFPLs, refinancing growth was higher in California overall and especially for California zip codes experienced higher CFPL house price growth. In other words, the CFPLs resulted in eased credit conditions in California, paving the way for those policies to have an impact on real economic activity.

11 Conclusion

This paper assesses the housing and broader economic effects of the California Foreclosure Prevention Laws, a unique set of 2000s crisis period mortgage modification programs that increased the cost and duration of the foreclosure process in an effort to encourage widespread modification of California mortgages. We find that the CFPLs significantly attenuated the decline of the California housing market, reducing the number of California homeowners that lost their homes by 380,000. Foreclosure reduction represents a key channel through which the CFPLs can affect house price growth. Indeed, the corresponding increase in housing wealth, using our most conservative estimates, was \$300 billion – a 6 percent increase. We also find that the CFPLs increased mortgage modifications while not adversely affecting the flow of credit to new borrowers.

A back of the envelope application of our estimates to Arizona and Nevada, two non-judicial

[50] Specifically, Mian et al. (2013) find using all zip codes an MPC out of changes in housing wealth of 0.018 (Table V, column (5)). They also show that there average MPC estimates fall by half with the inclusion of AZ, CA, FL, and NV (Table IV, columns (1) and (6)). Thus, our estimate of 0.006 is within an order of magnitude of the estimates from Mian et al. (2013).

foreclosure states whose housing markets were nearly indistinguishable from California's in the pre-treatment period, indicates that the CFPLs would have dramatically improved housing market conditions in these markets: 105,000 homes in Arizona and Nevada would have avoided REO foreclosure and housing wealth in these states would have increased by $40 billion.[51]

In addition to the salutary impact of the CFPLs on California housing markets, our results show that these policies had a positive effect on durable consumption as measured by auto sales. In particular, we estimate an elasticity of auto sales consumption with respect to CFPL housing wealth growth of 0.29. An easing of credit constraints represents an important channel through which CFPL house price gains may have affected the real economy.

All said, results of our analysis suggest that the CFPLs were substantially more effective than the US Government's HAMP Program for purposes of stabilizing housing markets and mitigating foreclosures. Further, contrary to concerns raised by policymakers regarding the likely transitory nature of foreclosure abeyance, our results suggest the gains to housing markets were long-lived.

[51]Foreclosure estimates use the 2007 population estimates for Arizona and Nevada and the CFPL REO foreclosure estimate in table 5. The total number of housing units is from the table S1101 of the 2007 1-Year ACS Community Survey and house prices are from 2008M06 from Zillow. (2,251,546*$209,700 + 954,067*$233,600)*0.0618 $\approx$ $43 billion.

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A Tables

Table 1: Sand States – Foreclosures and House Price Growth during the CFPL Period

	CFPL Treatment Period				
	AZ (1)	CA (2)	FL (3)	NV (4)	Diff-Diff (5)
Panel A: Foreclosures and the MDRI					
Forc Starts (% of All Loans)	22.75	15.96	24.31	31.25	-8.65
Prime Forc Starts (% of Prime Loans)	18.17	12.90	19.96	26.93	-7.31
Subprime Forc Starts (% of Subprime Loans)	57.94	45.71	50.91	66.36	-8.17
Zillow REO Forc per 10,000 people	961.76	640.26	NA	1194.28	-390.70
Growth in Mortgage Default Risk (MDRI; %)	7.05	-21.73	18.71	2.99	-36.43
Panel B: House Price Growth					
FHFA HP Growth (%)	-36.53	-20.19	-30.96	-43.56	13.16
Zillow All Homes HP Growth (%)	-38.91	-22.03	-33.39	-51.84	14.28
Zillow Bottom Tier HP Growth (%)	-60.62	-40.26	-53.48	-82.58	17.48
Zillow Top Tier HP Growth (%)	-30.29	-9.14	-22.62	-38.51	16.63

Notes: When foreclosure starts and FHFA returns represent the outcome variable (quarterly data), the CFPL treatment period is 2008Q3 to 2010Q4. For the Zillow data, the treatment period runs from 2008M07 to 2010M12 and growth in the MDRI is calculated from 2007-2010. Column (5) shows the difference-in-differences over the CFPL period for California relative to the average of AZ, FL, and NV, weighted by the number of households.

Table 2: Sand States – County and Zip Code Means and Diff-Diff Results

	CFPL Treatment Period				
	AZ (1)	CA (2)	FL (3)	NV (4)	Diff-Diff (5)
Panel A: County Foreclosures					
Zillow REO Forc per 10,000 people	939.06	596.78	NA	1221.93	-224.88*
	(134.25)	(60.90)	(-)	(143.04)	(77.34)
Panel B: Zip Code HP Growth					
Zillow All Homes HP Growth (%)	-41.61	-25.12	-39.50	-58.52	8.48*
	(1.48)	(0.58)	(0.82)	(2.47)	(0.89)
Panel C: County HP Growth					
Zillow All Homes HP Growth (%)	-38.57	-21.54	-33.85	-53.73	7.19*
	(5.07)	(2.25)	(2.34)	(3.31)	(3.01)
Zillow Bottom Tier HP Growth (%)	-59.39	-38.43	-55.89	-82.84	20.07*
	(9.81)	(2.65)	(4.07)	(5.65)	(4.88)
Zillow Top Tier HP Growth (%)	-29.69	-12.98	-21.99	-39.38	3.05
	(3.46)	(1.65)	(1.37)	(3.34)	(3.04)

Notes: This table shows outcome variable means for counties and zip codes within each state. Column (5) reports the difference-in-differences regression estimates (the coefficient on an indicator for California) using Sand State counties or zip codes as a control group. Controls in the regression include the pre-treatment house price growth (2004Q1-2008Q2), house price growth one year prior to the treatment (2007Q2-2008Q2), pre-treatment period return variance (2004Q1-2008Q2), unemployment rate in 2007, median income in 2007, and population density in 2007. All estimates are weighted by the number of households and White standard errors are in parentheses. An asterisk represents difference-in-differences significance at the 5 percent level.

Table 3: State-Level Synthetic Control Unit Weights

	Synthetic Control Region Weights
Panel A: Foreclosures and the MDRI	
Forc Starts (% of All Loans)	NV: 0.61; AZ: 0.18; OR: 0.15; MD: 0.06
Prime Forc Starts (% of Prime Loans)	NV: 0.62; MD: 0.20; AZ: 0.18
Subprime Forc Starts (% of Subprime Loans)	NV: 1.00; RI: 0.00
Zillow REO Forc per 10,000 people	NV: 0.66; MN: 0.34
Growth in Mortgage Default Risk (MDRI)	FL: 0.41; MI: 0.39; NV: 0.20
Panel B: Housing Returns	
FHFA Returns	NV: 0.88; MI: 0.12
Zillow All Homes Returns	FL: 0.34; NV: 0.34; RI: 0.33
Zillow Bottom Tier Returns	NV: 0.67; RI: 0.31; MN: 0.02
Zillow Top Tier Returns	NV: 0.33; RI: 0.28; MN: 0.17; WA: 0.13; FL: 0.09

Notes: The left column shows the outcome variable and the right column shows the contribution of each state to California's Synthetic Control. Only states with positive weights are listed.

Table 4: State-Level Synthetic Control Estimation Results

	RMSFE	CFPL Treatment Period			
		CA	Synth	Gap	Gap Percentile
	(1)	(2)	(3)	(4)	(5)
Panel A: Foreclosures and the MDRI					
Forc Starts (% of All Loans)	0.08	15.96	25.34	-9.38***	0.00
Prime Forc Starts (% of Prime Loans)	0.04	12.90	21.44	-8.54***	0.00
Subprime Forc Starts (% of Subprime Loans)	0.18	45.71	66.32	-20.61***	0.00
Zillow REO Forc per 10,000 people	1.07	640.26	948.03	-307.76***	0.00
Growth in Mortgage Default Risk (MDRI; %)	0.12	-52.08	-2.95	-49.12***	0.00
Panel B: House Price Growth					
FHFA HP Growth (%)	0.98	-20.19	-40.06	19.87***	100.00
Zillow All Homes HP Growth (%)	0.18	-22.03	-32.25	10.22***	100.00
Zillow Bottom Tier HP Growth (%)	0.25	-40.26	-61.20	20.94***	100.00
Zillow Top Tier HP Growth (%)	0.19	-9.14	-21.93	12.79***	96.97

Notes: The left column lists the outcome variable, RMSFE is the root mean-squared forecast error from the Synthetic control match during the pre-treatment period, the next two columns show the change in the outcome variable for California and its Synthetic Control during the CFPL treatment period (2008Q3-2010Q4; 2008M7-2010M12), and Gap is the difference between of the change in the outcome variable for Treated Unit (California) relative to its Synthetic Control. The growth in the MDRI is calculated from 2007-2010. The far right column shows the percentile of the Gap estimate relative to all placebo effects. One, two, or three asterisks indicates that the Gap estimate for the treated unit is the greater (lower) than the 85, 90, or 95th (5, 10, or 15th) percentiles of all estimated placebo effects.

Table 5: County and Zip Code Mean Synthetic Control Estimates in the CFPL Period

	Number of CA Regions	RMSFE Weighted Mean	Gap Weighted Mean	Standard Error of Gap Mean	Households with Gap Percentile > 85 (%)
	(1)	(2)	(3)	(4)	(5)
Panel A: County Foreclosures					
Zillow REO Forc per 10,000 people	21	1.29	-120.16**	(44.20)	66.60
Panel B: Zip Code HP Growth					
Zillow All Homes HP Growth	1195	0.72	9.58***	(0.38)	51.35
Panel C: County HP Growth					
Zillow All Homes HP Growth	39	0.48	6.18***	(1.33)	42.13
Zillow Bottom Tier HP Growth	38	0.41	9.60***	(2.99)	42.65
Zillow Top Tier HP Growth	39	0.48	2.85***	(1.03)	29.99

Notes: Column (1) shows the number of zip codes or counties in California with available data for the given outcome variable. Columns (2) and (3) list the mean pre-treatment RMSFEs and Gap Estimates, respectively, weighted by the number of households in 2007. In column (3), one, two, or three asterisks represents significance at the 10, 5, or 1 percent levels. Column (4) holds the standard error of the Gap weighted mean estimate. Column (5) shows the geographic salience of the CFPLs defined as percentage of California households covered by a county or zip code with a Gap Percentile greater than 85. For Zillow REO foreclosures, column (5) is 100 minus the Gap Percentile.

Table 6: Zip Code CFPL House Price Growth and Gap Percentile by California Region

	Central CA (1)	Inland Empire (2)	Los Angeles (3)	Northern CA (4)	Other CA (5)	South LA (6)
CFPL HP Growth	2.26	12.48	16.31	5.62	6.38	16.21
Percentage of Zip Codes with a Gap Percentile > 85	24.16	57.84	70.00	34.79	37.14	79.59
Total Number of Zip Codes	178	102	220	457	140	98

Notes: For each region, the mean weighted CFPL Gap in House Price Growth and the percentage of zip codes with a Gap Percentile greater than 85. The bottom panel shows the total number of zip codes in each region. We define the these regions as follows from South to North: South LA is north of south San Clemente, South of where I-5 meets CA-91, and West of where I-605 meets the CA-60, lat > 33.392089 & lat < 33.856324 & long < -117.590565; Los Angeles is North of where I-5 meets CA-91, South of Ojai, and west of where I-605 meets CA-60, lat > 33.856324 & lat < 34.464635 & long < -118.027303; the Inland Empire is East of where I-605 meets CA-60, west of were CA-60 meets I-10, south Ojai, and north where I-5 meets CA-91, lat > 33.856324 & lat < 34.464635 & long > -118.027303 & long < -116.990628, Central California is North of Ojai and south of San Jose, lat > 34.464635 & lat < 37.243092; Northern California is north of San Jose, lat > 37.243092. Other includes all zip codes not in the defined regions. We sort zip codes into these regions using their average latitudes and longitudes.

Table 7: Non-Judicial States – County and Zip Code Synth Estimates in the CFPL Period

	Number of CA Regions (1)	RMSFE Weighted Mean (2)	Gap Weighted Mean (3)	Standard Error of Gap Mean (4)	Households with Gap Percentile > 85 (%) (5)
Panel A: County Foreclosures					
Zillow REO Forc per 10,000 people	21	1.34	-139.29***	(38.59)	76.45
Panel B: Zip Code HP Growth					
Zillow All Homes HP Growth	1195	0.73	16.29***	(0.49)	65.29
Panel C: County HP Growth					
Zillow All Homes HP Growth	39	0.37	6.42***	(1.40)	51.79
Zillow Bottom Tier HP Growth	38	0.42	7.25**	(3.50)	39.14
Zillow Top Tier HP Growth	39	0.50	3.42***	(0.85)	33.86

Notes: The control group consists of counties or zip codes located in only **non-judicial states**. Column (1) shows the number of zip codes or counties in California with available data for the given outcome variable. Columns (2) and (3) list the mean pre-treatment RMSFEs and Gap Estimates, respectively, weighted by the number of households in 2007. In column (3), one, two, or three asterisks represents significance at the 10, 5, or 1 percent levels. Column (4) holds the standard error of the Gap weighted mean estimate. Column (5) shows the geographic salience of the CFPLs defined as percentage of California households covered by a county or zip code with a Gap Percentile greater than 85. For Zillow REO foreclosures, column (5) is 100 minus the Gap Percentile.

Table 8: Difference-in-Differences Regressions in the Probability of Mortgage Modification

	Dependent variable:			
	Prob(Modify | Delinquent)			
	(1)	(2)	(3)	(4)
California	-0.002^{*}	-0.003^{*}	-0.003^{*}	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)
CFPL	-0.017^{***}	-0.017^{***}	-0.018^{***}	-0.018^{***}
	(0.003)	(0.003)	(0.003)	(0.003)
California x CFPL	0.004^{***}	0.005^{***}	0.005^{***}	0.005^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Loan-level Controls?	No	Yes	Yes	Yes
House Price Controls?	No	No	Yes	Yes
Macro Controls?	No	No	No	Yes
Observations	76,930	73,230	73,114	73,114
Sample	AZ, CA,NV	AZ, CA,NV	AZ, CA,NV	AZ, CA,NV
Prob(Modify | Delin, Pre-CFPL)	0.017	0.017	0.017	0.017

Notes: Difference-in-differences probit regressions using Fannie Mae loan performance data. The dependent variable takes a value of one if a mortgage has been modified and zero otherwise and the coefficients reported in the table are the marginal effects (average partial effects). The sample is a repeated cross section of delinquent loans during the pre-CFPL and CFPL periods. The pre-CFPL period ranges from August 2007 to February 2008 and the CFPL treatment period is from August 2008 to February 2009. For the pre-CFPL period, the sample is restricted to loans that are 30, 60, or 90 days delinquent at any point in 2007Q2. For the CFPL period, the sample is restricted to similarly delinquent loans in 2008Q2. California takes a value of one if the loan is associated with a home in California and zero otherwise. CFPL equals zero for 2007M08 - 2008M02 (pre-CFPL period) and one for 2008M08 - 2009M02 (CFPL period). Modification is assumed to be an absorbing state and thus loans modified during the pre-CFPL period are removed from consideration for the CFPL period. Loan-level controls include a factor variable for the delinquency status at the start of the pre-CFPL or CFPL periods. Loan-level controls also include a factor for the origination year, the original interest rate, the borrower credit score at the time of origination, the debt-to-income ratio at origination, and the log of the original loan amount. Zillow house price controls in columns (2) and (3) are measured at the 3 digit zip code level and include the log of the median house price in 2007Q1, the house price growth in 2007, and the housing return variance from 2004Q1 - 2008Q2. The macro controls in columns (3) include the county unemployment rate in 2007 and the 3 digit zip code income per household in 2007. The bottom row shows the average probability of modification, given delinquency, in the pre-CFPL period. Heteroskedasticity robust standard errors are in parentheses. One, two, or three asterisks represents significance at the 10, 5, or 1 percent levels, respectively.

Table 9: Probability of Denial and Loan Volume Growth after the CFPLs

	Dependent variable:					
	Prob(Deny)		Loan Growth ($)		Loan Growth (Num)	
	(1)	(2)	(3)	(4)	(5)	(6)
California	−0.001	−0.049***	0.096***	0.039*	0.170***	0.102***
	(0.001)	(0.001)	(0.027)	(0.022)	(0.023)	(0.018)
Sample	AZ,CA, FL,NV Loan Level	CA,CO, NY,TX Loan Level	AZ,CA, FL,NV Zip Code	CA,CO, NY,TX Zip Code	AZ,CA, FL,NV Zip Code	CA,CO, NY,TX Zip Code
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Estimation Method	Probit	Probit	OLS	OLS	OLS	OLS
Observations	797,732	1,278,510	1,086	1,668	1,086	1,668
R^2			0.709	0.693	0.751	0.756

Notes: Regressions of the probability of mortgage denial and zip code level loan volume growth on an indicator for California and controls. In columns (1) and (2), the dependent variable takes a value of one if the mortgage application was denied and zero otherwise and the coefficients reported in the table are the marginal effects (average partial effects). California takes a value of one for California and zero otherwise. Controls in columns (1) and (2) include the log of applicant income and loan amount; Zillow house price returns and IRS income and population growth in the year before the loan application was submitted; and factor variables for applicant race and applicant sex. The samples include only loans not sold to GSEs in AZ, CA, FL, and NV (column 1) and CA, CO, NY, and TX (column 2) from 2009 to 2014. Columns (3) - (4) and (5) - (6) show regressions where dollar loan volume growth or the growth in the number of loans represents the dependent variable. The sample is restricted to loans not sold to GSEs. The key right-hand-side variable of interest is an indicator that takes a value of 1 for California. The data for these regressions are at the zip code level. Controls include applicant income growth and IRS income and population growth as well as Zillow zip code level house price growth for 2008-2009, 2010-2011, and 2012-2014. The regressions in columns (3) - (6) are weighted by the number of households. Heteroskedasticity-robust standard errors are in parentheses. One, two, or three asterisks represents significance at the 10, 5, or 1 percent levels, respectively.

Table 10: CFPL Treatment Effect – House Price and Auto Sales Growth

	Dependent variable:			
	CFPL Gap in Auto Sales Growth			
	(1)	(2)	(3)	(4)
CFPL Gap in House Price Growth	0.285^{*}	0.103	0.268^{**}	0.206
	(0.145)	(0.184)	(0.121)	(0.144)
(CFPL Gap in House Price Growth)^2		0.023^{*}		
		(0.012)		
Pre-Treatment Housing Return Variance			-6.074^{***}	-4.572
			(2.150)	(3.016)
HP Growth 1 Year Prior to CFPLs			-0.712^{**}	-0.597^{*}
			(0.331)	(0.346)
Median Household Income in 2007 ($000s)				-0.048
				(0.130)
Unemployment Rate in 2007				-1.090
				(1.494)
Observations	39	39	39	39
R^2	0.117	0.196	0.247	0.265

Notes: Regressions of the Synthetic Control Gap in the growth of auto sales on the CFPL All Homes House Price Growth. The CFPL Synthetic Control counterfactuals for auto sales are constructed using only a sample of non-judicial foreclosure states. Regressions are weighted by the number of households and White standard errors are in parentheses. One, two, or three asterisks represents significance at the 10, 5, or 1 percent levels, respectively.

Table 11: CFPL Treatment Effect – Average Marginal Propensity to Consume out of CFPL House Price Increases

	Dependent variable:			
	CFPL Gap in ΔAuto Spending ($000s)			
	(1)	(2)	(3)	(4)
CFPL Gap in ΔHome Value ($000s)	0.006**	0.002	0.006***	0.006**
	(0.003)	(0.004)	(0.002)	(0.003)
(CFPL Gap in ΔHome Value, $000s)^2		0.0001		
		(0.0001)		
Pre-Treatment Housing Return Variance			−0.490**	−0.296
			(0.193)	(0.301)
HP Growth 1 Year Prior to CFPLs			−0.068**	−0.056*
			(0.029)	(0.033)
Median Household Income in 2007 ($000s)				0.004
				(0.011)
Unemployment Rate in 2007				−0.083
				(0.141)
Observations	39	39	39	39
R^2	0.139	0.192	0.245	0.277

Notes: Regressions of the CFPL Gap in the change in auto spending on the CFPL Gap in the change in home values. Δ represents change in thousands of dollars. The CFPL Synthetic Control counterfactuals for auto sales are constructed using only a sample of non-judicial foreclosure states. White standard errors are in parentheses and all regressions are weighted by the number of households. One, two, or three asterisks represents significance at the 10, 5, or 1 percent levels, respectively.

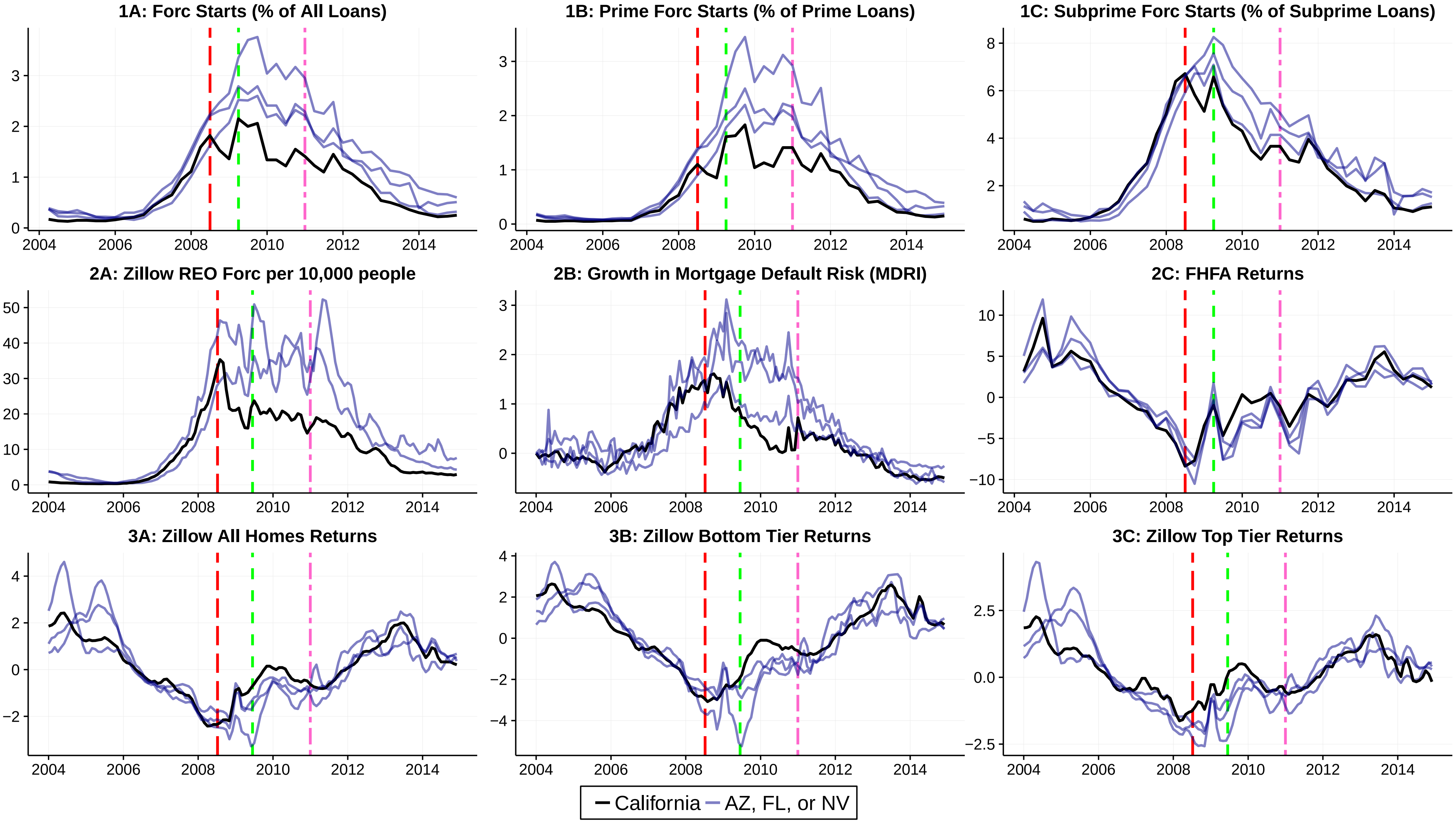
Table 12: Zip Code Growth in Refinancing Volume

	Dependent variable:	
	Growth in Refinancing Volume	
	(1)	(2)
California	0.193***	
	(0.021)	
CFPL Gap in House Price Growth		0.016***
		(0.002)
Sample	AZ,CA, FL,NV	CA
Controls?	Yes	Yes
Observations	2,129	1,087
R^2	0.787	0.531

Notes: The dependent variable is the log difference in HMDA dollar refinancing volume in 2009 and 2010 relative to 2007. In column (1), the right hand side variable of interest is an indicator that takes the value one for zip codes in California and zero otherwise. The sample for column (1) includes zip codes in Arizona, California, Florida, and Nevada. Column (2) shows the coefficient on the CFPL Gap in House price growth where the sample is limited to zip codes in California. In columns (1) and (2), controls include IRS Household Income in 2007, IRS income and population growth in from 2008-2009 and 2010-2011. The regression in column (1) also includes controls for zip code house price growth from 2008-2009 and 2010-2011. White heteroskedasticity robust standard errors are in parentheses. The Regressions are weighted by the number of households. One, two, or three asterisks represents significance at the 10, 5, or 1 percent levels, respectively.

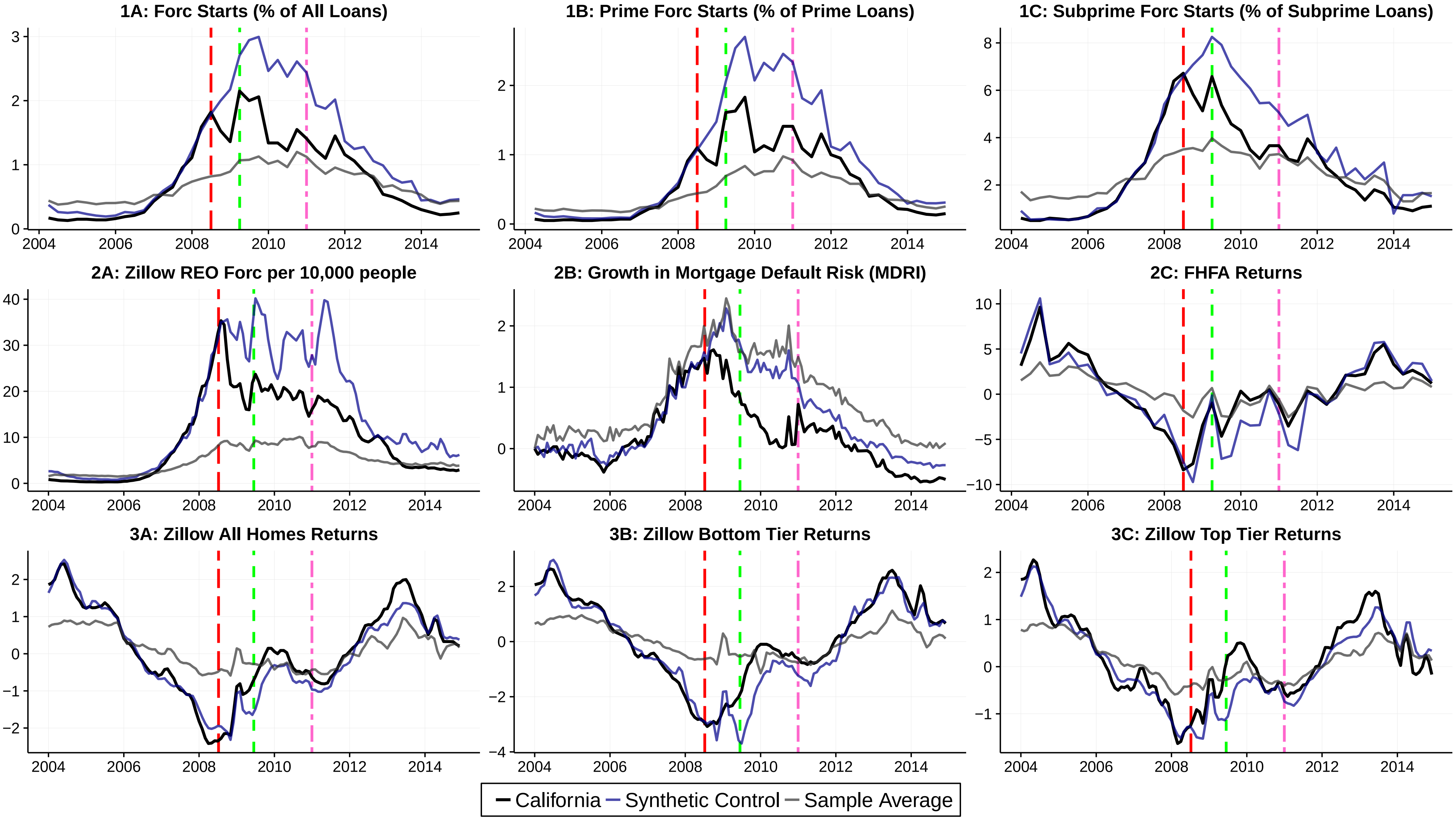
B Figures

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



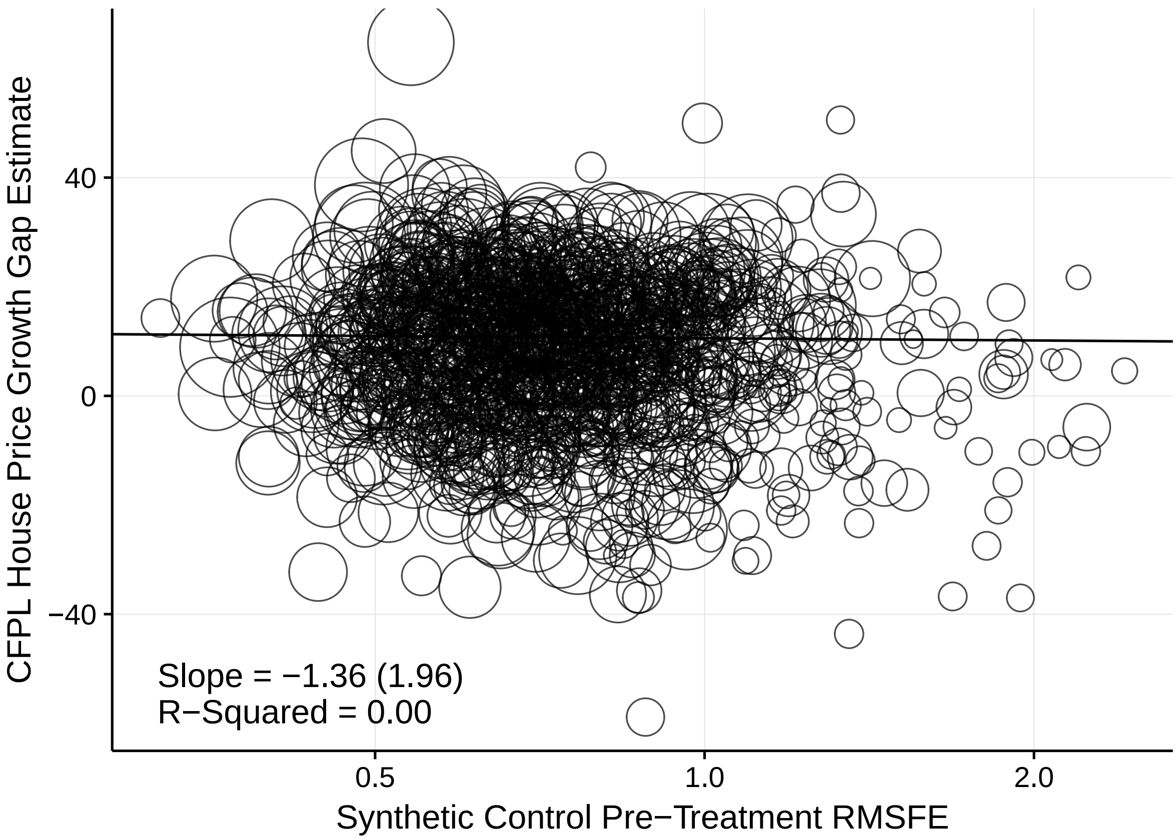
Notes: Plots of foreclosure starts, mortgage distress, and housing returns for Arizona, California, Florida, and Nevada. The black line is California, the blue lines represent Arizona, Florida, or Nevada. The long-dashed-red vertical line signifies the passage CA-1137 in 2008Q3 (2008M07); the short-dash-green vertical line represents the CFPA implementation date in 2009Q2 (2009M06), and the two-dashed-pink line is the sunset date for the CFPA and the end of the policy period in 2010M12 (2010Q4).

Figure 2: State-Level Synthetic Control Results: Foreclosures, Mortgage Distress, and Housing Returns



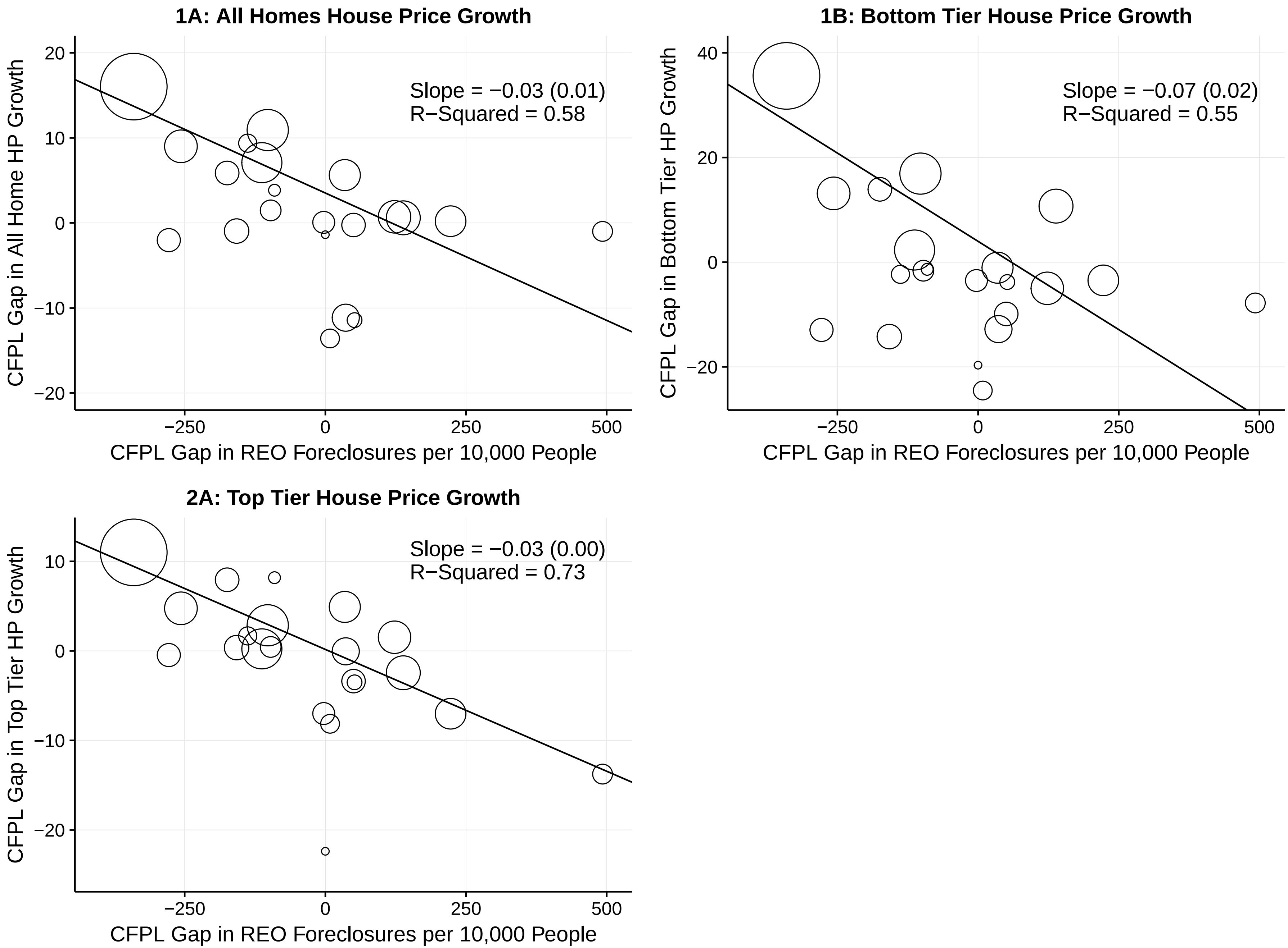
Notes: The black line is California, the blue line is the Synthetic Control, and the gray line represents the unweighted sample average. The long-dashed-red vertical line signifies the passage CA-1137 in 2008Q3 (2008M07); the short-dash-green vertical line represents the CFPA implementation date in 2009Q2 (2009M06), and the two-dashed-pink line is the sunset date for the CFPA and the end of the policy period in 2010M12 (2010Q4).

Figure 3: Synthetic Control CFPL RMSFEs and Gap Estimates for California Zip Codes



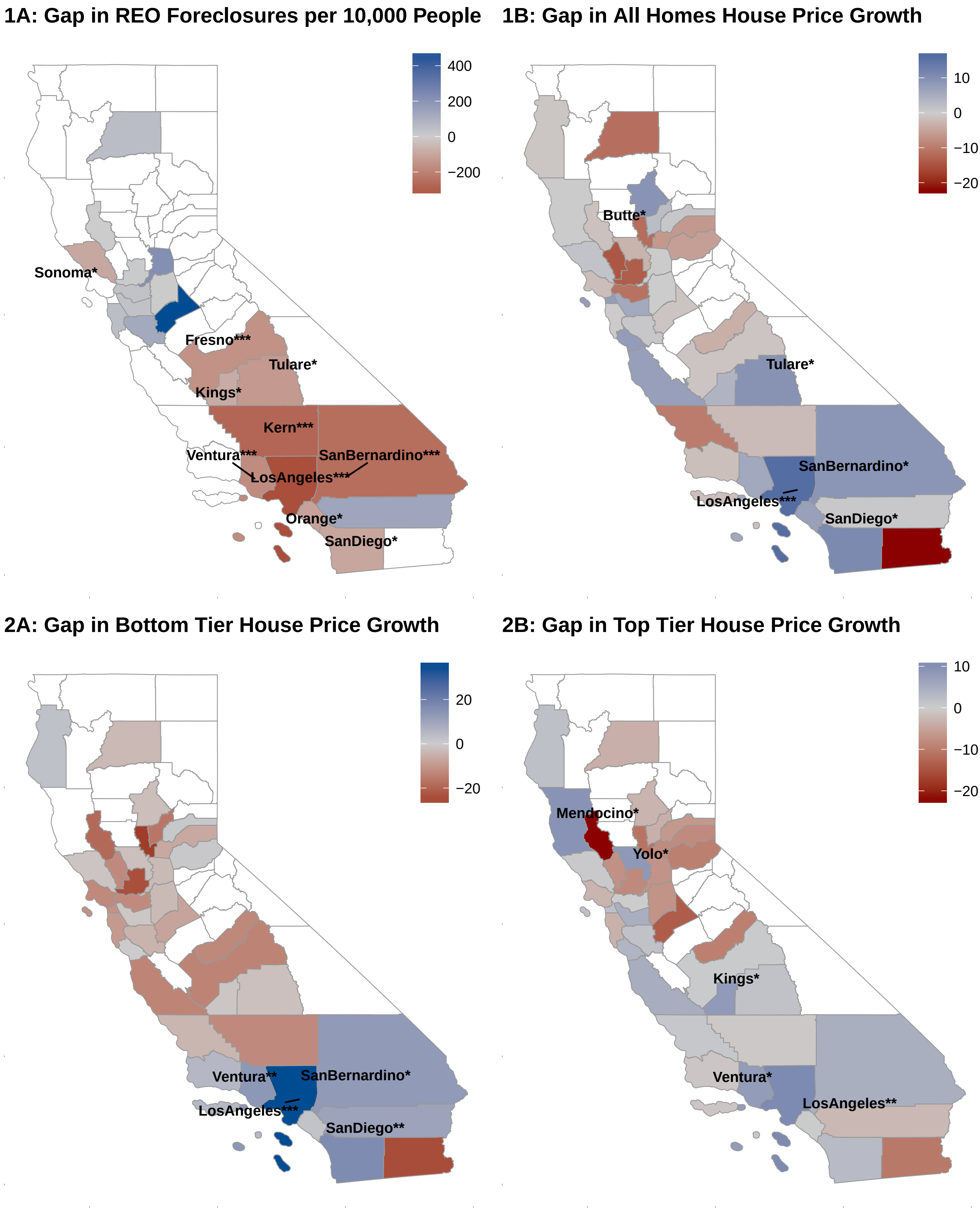
Notes: The scatterplot and the regression line are weighted by the number of households. The horizontal axis is in logs. White standard errors are in parentheses.

Figure 4: The CFPL Treatment Effect – County Foreclosures and House Price Growth



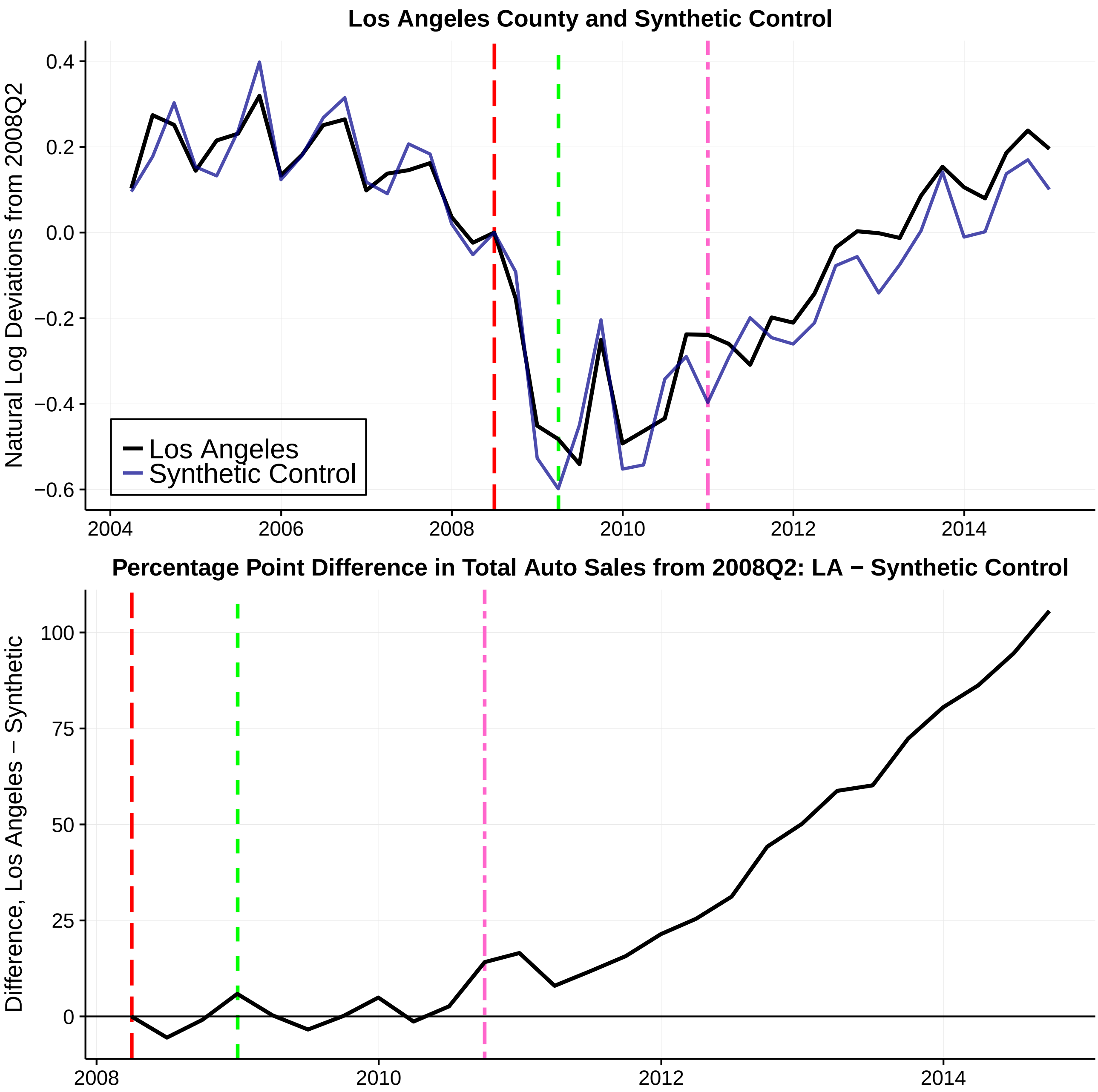
Notes: The CFPL gap in REO Foreclosures per 10,000 people and the CFPL Gap in All Homes House Price Growth. The scatterplot and the regression line are weighted by the number of households. White standard errors are in parentheses.

Figure 5: County Level Gaps in CFPL Foreclosures and House Price Growth



Notes: Choropleth plots of county-level REO foreclosures per 10,000 people and house price growth during the CFPL treatment period. Counties names are printed on the plot if Gap Percentile is greater than 85 (less than 15 foreclosures) and 1, 2, or 3 asterisks indicates a Gap Percentile greater than 85, 90, or 95 (less than 15, 10, or 5 for foreclosures). Counties in white have no data.

Figure 6: Auto Sales – Los Angeles County vs. Synthetic Control



Notes: New auto registrations for Los Angeles and its Synthetic Control. In the top panel, each line is in log deviations from 2008Q2. The bottom panel shows the difference in the percentage change of total auto sales relative to 2008Q2. The long-dashed-red vertical line signifies the passage CA-1137 in 2008Q3; the short-dash-green vertical line represents the CFPA implementation date in 2009Q2, and the two-dashed-pink line is the sunset date for the CFPA and the end of the policy period in 2010Q4.

C Appendix: Eligibility, Affordability, Sustainability, Timeline of the CFPA

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

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

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

D Appendix: Synthetic Control Robustness

Table D1: State-Level Synthetic Control Robustness – Alternative Control Groups

	Min Absolute Gap
Panel A: Foreclosures and the MDRI	
Forc Starts (% of All Loans)	-12.66
Prime Forc Starts (% of Prime Loans)	-8.69
Subprime Forc Starts (% of Subprime Loans)	-16.21
Zillow REO Forc per 10,000 people	-506.43
Growth in Mortgage Default Risk (MDRI)	-34.38
Panel B: House Price Growth	
FHFA Returns	8.88
Zillow All Homes Returns	14.79
Zillow Bottom Tier Returns	24.02
Zillow Top Tier Returns	12.68

Notes: Robustness Check for the state-level Synthetic Control results. Each state is iteratively eliminated from the donor pool as a potential control. For each of these alternative control groups the Synthetic counterfactual is then computed. The table reports the minimum absolute value (closest to zero) of these Gap estimate.

E Appendix: Tables

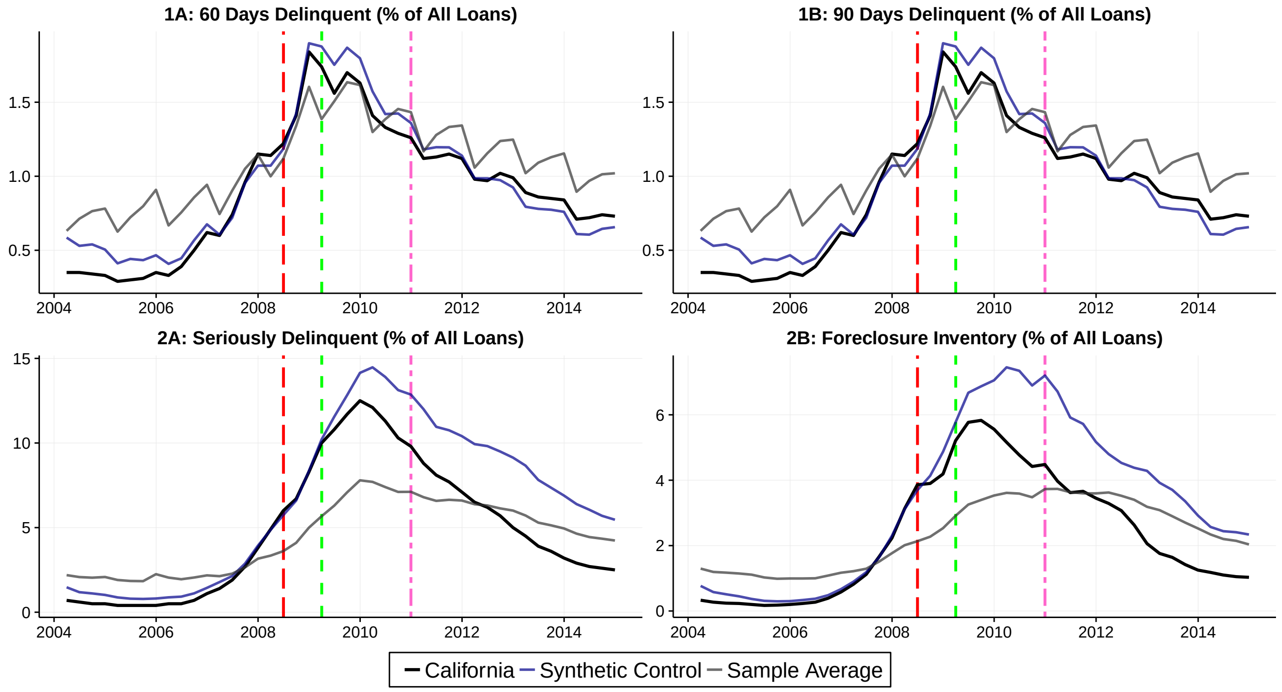
Table E1: Summary Statistics For Loan Modification Difference-in-Differences Control and Treatment Groups

	Pre-CFPL				CFPL Treatment Period			
	AZ, NV		CA		AZ, NV		CA	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
FICO Credit Score	671.04	(57.51)	672.83	(58.82)	679.40	(55.15)	679.84	(56.57)
Debt-to-income Ratio	37.35	(11.96)	38.27	(12.09)	39.31	(11.75)	40.21	(11.79)
Interest Rate	6.30	(0.63)	6.04	(0.55)	6.32	(0.53)	6.09	(0.52)

Notes: Summary Statistics of loan-level borrower characteristics at the time of origination in the pre-CFPL and CFPL periods for the treatment group (California) and the control group (Arizona and Nevada). The sample is a repeated cross section of delinquent loans during the pre-CFPL and CFPL periods. The pre-CFPL period ranges from August 2007 to February 2008 and the CFPL treatment period is from August 2008 to February 2009. For the pre-CFPL period, the sample is restricted to loans that are 30, 60, or 90 days delinquent at any point in 2007Q2. For the CFPL period, the sample is restricted to similarly delinquent loans in 2008Q2.

F Appendix: Figures

Figure F1: State-level Synthetic Control Results – Delinquencies and Foreclosure Inventory



Notes: The black line is California, the blue line is the Synthetic Control, and the gray line represents the unweighted sample average. The long-dashed-red vertical line signifies the passage CA-1137 in 2008Q3 (2008M07); the short-dash-green vertical line represents the CFPA implementation date in 2009Q2 (2009M06), and the two-dashed-pink line is the sunset date for the CFPA and the end of the policy period in 2010M12 (2010Q4).