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

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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. Using the Synthetic Control Methodology, we find that the CFPLs prevented 330,000 California foreclosures, equivalent to a 25% 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 the these policies increased house prices by 5 percent and in doing so created \$250 billion of housing wealth. Findings further indicate that these gains in housing wealth did not translate 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.

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1 Introduction

In the wake of the 2000s boom-period run-up in house prices, California in 2005 accounted for one-quarter of US housing wealth.[1] But as the 2006 boom turned into the 2008 bust, house prices in the state fell by 30 percent and 1 million California borrowers lost their homes to foreclosure.[2] In an effort to contain mounting home foreclosures both in California and beyond, the Federal Government enacted Home Affordable Modification Program (HAMP), which sought to modify distressed mortgages by offering financial incentives to both homeowners and lenders.[3] Yet, as shown by Agarwal et al. (2012), HAMP had little economic impact as many mortgage lenders lacked the infrastructure to modify loans on a large scale.[4] At the epicenter of the housing bust, the State of California pursued an alternative policy approach to aid distressed borrowers and limit substantial foreclosures. Rather than offering financial incentives to modify loans as in HAMP, the California policies instead imposed foreclosure moratoriums and incremental pecuniary costs on lenders to encourage their widespread 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.[5] Unlike HAMP, there has been little attention paid to and no prior evaluation of alternative policy efforts that used increased foreclosure costs to stem the housing and foreclosure 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 an 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) for the property. In July of 2008 and in the midst of a severe housing crisis, the state passed the first of the CFPLs, Senate Bill 1137 (SB-1137).[6] This bill, which immediately went into effect, prohibited mortgage lenders and

[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] California house prices from the FRED economic database. The number of California foreclosures is from Zillow.

[3] The Federal Government also implemented other housing policy during the crisis. These programs are discussed below in section 2.

[4] Specifically, Agarwal et al. (2012) find HAMP reached just a third of the targeted 3-4 million households.

[5] Inaction by both lenders and borrowers are both potential constraints in the implementation of mortgage modification programs. Section 2 discusses these issues in more detail.

[6] `ftp://www.leginfo.ca.gov/pub/07-08/bill/sen/sb_1101-1150/sb_1137_bill_20080708_chaptered.html`

servicers (henceforth, lenders) from issuing a NOD until 30 days after informing the homeowner of potential foreclosure alternatives either by telephone or in person.[7] The homeowner then had the right within 14 days of first contact to schedule a second telephonic meeting with the lender to discuss foreclosure 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* after the NOD unless the lender had implemented a comprehensive mortgage modification program. The adequacy of the mortgage modification programs was determined by the State of California based on debt-to-income targets and potential interest rate or principal payment reductions.[8] 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 and extensive 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 and important opportunity to use cross-sectional variation to assess the housing and related economic effects of crisis-period law which sought to encourage widespread mortgage modification. Using the Synthetic Control Methodology (SCM) and data at various levels of geography, we find that the CFPLs reduced (Real Estate Owned (REO)) foreclosures by 25 percent and hence prevented 330,000 California borrowers from losing their homes. The CFPLs also mitigated prime and subprime foreclosure starts and ameliorated household mortgage default risk.

Our most conservative estimate of the relative gain in California house prices due to the CFPLs is 5 percent – equivalent to a $250 billion increase in housing wealth.[9] Our median and preferred estimate of the house price appreciation due to the CFPLs, found using highly disaggregated zip code level data, is 9.5 percent. These effects were largely concentrated in

[7] If lenders could not reach homeowners, they had to undertake "due diligence" in their attempts to contact the homeowner. See section 2 for more details.

[8] See section 2 for more details.

[9] 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*0.0479 $\approx$ $250 billion.

the hard-hit areas of Southern California. Indeed, using zip code level data, we find that the CFPLs caused a 15.8 percent relative house price increase in the Southern California Coastal and Inland Region.

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 (ARRA) and social transfers, totaled $114 billion.[10] The magnitude of the housing stimulus created by the CFPLs ($250 billion using our most conservative estimate) was thus 220 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 decline in California housing markets during the policy period.

Despite the aforementioned bolstering of housing markets, the broader economic effects of the CFPLs were limited as we find no evidence of an increase in durable consumption as measured through auto sales. These findings are consistent across California and in areas where the CFPLs housing impacts were highly beneficial. For example, in Los Angeles County, the CFPLs increased house prices by 15 percent and created $188 billion of housing wealth. This rise in house prices was evidenced in 70 percent of Los Angeles zip codes. Yet we find no increase in Los Angeles County auto sales per capita due to the introduction of the CFPLs, suggesting a muted impact of the CFPLs beyond the housing sector.

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.[11] This was the prevailing view among leading US policymakers during the crisis.[12] Further, other government loan modification programs enacted during the crisis were largely unsuccessful (Agarwal et al. (2012)). 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

[10] 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.

[11] "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.

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

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.[13] The economic justification for these house price gains in areas with with lower foreclosures is based on foreclosure externalities or theories of foreclosure induced fire sales. With regard to foreclosure externalities, a large literature contends that a foreclosure negatively affects 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 on their homes (Geradi et al (2015)).[14] 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).[15] 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 that entered the foreclosure process.[16] This is what we find in our empirical work: The CFPLs lowered mortgage defaults and stemmed 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 reversed in later periods, meaning that the CFPLs did not induce lingering delinquencies, prolong the crisis, or simply delay foreclosures until a later date.

The broader economic impacts of the CFPLs were also unclear ex ante as there has been little agreement on the marginal propensity to consume (MPC) out of an increase in housing wealth during a severe housing downturn. Tim Geithner (2014), the US Treasury Secretary from 2009-2013, contended that the MPC out of an increase in housing wealth during the crisis would have been a mere 0.01 or 0.02. 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 depreciated, illiquid, hard-to-value asset is less steep. Specifically, increases in housing wealth may affect consumption through a wealth channel or a collateral constraints

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

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

[15] See also Shleifer and Vishny (1992), Kiyotaki and Moore (1997), Krishnamurthy (2003, 2010), and Lorenzoni (2008).

[16] Bolton and Rosenthal (2002). See section 3 for a more detailed overview.

channel (e.g. equity withdrawal or refinancing). Yet note that the CFPLs lessened the decline in house prices – house prices still fell during the treatment period, but the drop was less steep due to the introduction of the CFPLs. Thus, the effects of the wealth channel are likely to be small (see also Buiter (2008) and Sinai and Souleles (2013)), while in the case of California the collateral channel is likely to be impotent as homeowners, especially those with non-standard mortgages, may have had difficulty refinancing homes with already depressed prices in the private market. Indeed, 60 percent of all mortgages originated in California at the peak of the boom were interest only and thus unlikely candidates for subsequent refinancing during the housing bust.[17] In contrast, some recent academic research papers suggest that the MPC out of housing wealth to ranges from 0.05 to 0.10.[18] Our results suggest that the CFPLs, while buttressing distressed housing markets, had little impact on durable consumption measured through auto sales.

The remainder of this paper is organized as follows: Section 2 provides of a detailed overview of the CFPLs; in section 3 we outline our main empirical hypotheses; the data and econometric methodologies are described in sections 4 and 5; section 6 provides a preliminary differences-in-differences analysis using just the Sand States that were highly exposed to the housing boom–Arizona, California, Nevada, and Florida; the main Synthetic Control results are in 7; and section 10 concludes.

2 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 provide mortgage lenders with incentives to modify loans by increasing the pecuniary and time costs of foreclosure. The following two sections discuss these laws in greater detail.

[17]Furhter, Mian et al. (2013) find that regions that suffered declines in housing wealth also experienced tighter credit constraints during the Great Recession. In California, 60 percent of all new mortgages originated in 2005 were non-standard interest only loans and anecdotal evidence suggests that these borrowers had trouble refinancing such loans: http://www.sfgate.com/news/article/High-interest-in-interest-only-home-loans-2634044.php and http://www.nytimes.com/2009/09/09/business/09loans.html?pagewanted=all&_r=0.

[18]See Bostic et al. (2009) for an overview.

2.1 California Senate Bill 1137 (SB-1137)

California Senate Bill 1137 (SB-1137) was signed into law on July 8, 2008 and mandated that mortgage lenders operating in California delay filing a notice of default (NOD), the first step in the foreclosure process, until 30 days after contacting the homeowner with information on foreclosure alternatives.[19] 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. (2012)). 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.[20] The sunset date for SB-1137 was January 1, 2013.

Prior to the enactment of SB-1137, existing law required only that the lender file a NOD with the appropriate county recorder and then mail the NOD to the mortgage borrower. In this letter, lenders were not obligated to provide any 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 pecuniary and time costs of foreclosure (for example, by requiring maintenance of vacant foreclosed properties), the State of California sought to change the net present value calculation of foreclosure versus mortgage modification.

2.2 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.[21] The aim of the 2009 California Foreclo-

[19] ftp://www.leginfo.ca.gov/pub/07-08/bill/sen/sb_1101-1150/sb_1137_bill_20080708_chaptered.html

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

[21] Further, 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

sure Prevention Act (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 handing defaulted homeowners a notice of sale (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 period between the NOD and the NOS. Thus, under the CFPA, lenders that had not implemented loan modification programs meeting the CFPA regulations were required to wait a total of six months between the NOD and the NOS.[22]

Mortgage lenders who implemented an acceptable mortgage modification program were exempted from the additional 90 day CFPA foreclosure moratorium. To obtain this exemption, a lender's loan modification program was required to 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. Further, an adequate modification program was required 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. Note that lenders participating in the federal government's Home Affordable Modification Program (HAMP) were considered to be in compliance with the CFPA and thus are exempt from the extra 90 day foreclosure moratorium under the law.[23]

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.

[22]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.

[23]The CFPA also outlined long-term sustainability goals regarding the performance of mortgage loans

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 that the CFPA may have provided stronger incentives for lenders to implement a mortgage modification program.[24]

While accurate data on mortgage workouts under the CFPA 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.[25] 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 the federal government's Home Affordable Modification Program (HAMP). Hence, HAMP accounted for just 35 percent of the modified mortgages over the foregoing sample period. Interestingly, Agarwal et al. (2012) find nationally that HAMP reached just one-third of its targeted homeowners, implying that the additional stipulations mandated by the CFPA may have allowed modifications in California to reach the levels targeted by the federal program.

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

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.

[24]Also, California law previously required lenders to contact borrowers 30 days prior to filing a NOD. With the implementation of the CFPA, lenders were further obligated to include information on their mortgage modification programs with this initial contact.

[25]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.

until maturity.[26]

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 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 may have preferred the additional 90 days in foreclosure so they could avoid taking possession of non-performing properties during the height of the foreclosure crisis.

2.3 Comparison to Other Mortgage Modification Programs

During the Great Recession, other public and private entities also implemented mortgage modification programs or foreclosure moratoriums. In October 2008, Countrywide entered into a multi-state settlement and agreed to modify all subprime mortgages that were at least 60-days delinquent; in March 2009, the federal government instituted the HAMP and HARP programs; and several mortgage banks, including Bank of America and JPMorgan, instituted a foreclosure moratorium in October 2010 in light of the filing of flawed foreclosure documents.[27] As noted by Mayer et al. (2014), Agarwal et al. (2012), and Agarwal et al. (2015), these modification programs were national in nature and thus there is no reason to expect these programs to differentially affect comparable homeowners across states. This implies that these programs do not inhibit our ability to use cross-sectional state-level variation to assess the effects of the CFPA.

HAMP, the largest federal mortgage modification program, was similar in aim to SB-1137 and the CFPA, but the two policy prescriptions differed dramatically in their implementation. HAMP offered cash incentives for lenders and mortgage investors to engage in loan modification and subsidized mortgage borrowers who remained current on their mortgages thereafter. In marked contrast, the CFPA, for example, placed a 90 day foreclosure moratorium on lenders who did not implement an acceptable mortgage modification program. Thus, HAMP and the CFPA programs adopted different carrot and the stick approaches, respectively, in

[26] See California (2010) for more details.

[27] `http://www.nytimes.com/2010/10/08/business/08frozen.html?_r=0`. Other mortgage banks including Citigroup and Indymac also implemented also implemented mortgage modification programs. See Mayer et al. (2014) and Agarwal et al. (2012) for more details

their efforts to encourage mortgage modification: HAMP offered the "carrot" of subsidies to lenders who participated in the HAMP program, whereas the CFPA used the "stick" of an extra cost (in terms of the foreclosure moratorium) on lenders lacking an adequate mortgage modification program. Thus, even in the event of inaction by lenders on specific loans, households received the CFPA treatment. In their analysis of HAMP, Agarwal et al. (2012) find that the federal loan program reached just one-third of its targeted three to four million targeted households and that HAMP adversely reduced mortgage renegotiations outside of the government program. Further, as shown in An et al. (2016), HAMP may have inadvertently incented borrowers to enter into default in order to qualify for loan modification. While we do not have access to data regarding modifications in California prior to the CFPA, survey data from lenders who implemented mortgage workout programs under the CFPA indicate that a substantial number of modifications took place outside of the HAMP program. Further, as documented above, the vast majority of lenders did not receive an exemption from the CFPA foreclosure moratorium via the HAMP program, suggesting that the CFPA possibly offered stronger incentives than HAMP.

Mortgages sold into private securitization faced higher foreclosure rates and were less likely to modified as the companies servicing securitized mortgages have financial incentives to pursue foreclosure rather modification.[28] Hence, securitization further impaired the effects of HAMP. Yet the foreclosure moratoriums pursued under the CFPLs applied to both securitized and non-securitized mortgages; thus, unlike HAMP, households whose mortgages were sold into securitization also received the policy treatment.[29]

In addition to HAMP, the Federal Government also pursued HARP, a mortgage refinancing program that offered government guarantees via the GSEs to facilitate the refinancing of insufficiently collateralized *conforming* mortgages. Yet at the peak of the boom, over 60 percent newly originated in California were interest only and thus not eligible for the HARP program.[30] This means that the vast majority of California borrowers who were likely facing underwater mortgages were not eligible for the HARP program. Antithetically, mortgage borrowers received the CFPL treatment regardless of their loan type. In reviews of HARP,

[28]Piskorski et al. (2010), Agarwal et al. (2011), and Kruger (2016).

[29]Mortgages where the pooling or servicing agreement prohibited modification were not subject to the CFPA program requirements. Yet, as noted by Kruger (2016), only a minority of servicing agreements prohibited modification.

[30]See "High interest in interest-only home loans." San Francisco Chronicle. May 20, 2005.

recent research suggests that that the HARP refinance led to lower interest rates, reduced monthly payments, and increased durable consumption via auto sales.[31]

In order for HAMP and HARP to be efficacious *both* lenders and mortgage borrowers needed to take steps towards mortgage modification. Not only did lenders generally lack the infrastructure to modify mortgages on a large scale, but anecdotal evidence also suggests that mortgage borrowers may have been inattentive to letters and notices regarding mortgage modification programs.[32] The CFPLs circumvent these issues by applying the treatment to all households, regardless of actions taken by either or lenders or borrowers, and by adding stipulations on contact procedures for mortgage borrowers.

At the state-level Massachusetts, on May 1, 2008, passed a "right-to-cure" law that prevented lenders from initiating the foreclosure process for an extended period of time after the borrower defaulted on a loan. In an analysis of this law, Gerardi et al. (2013) find that this law delayed, but did not prevent foreclosure. Hence, due to the law, borrowers lingered in persistent delinquency but were ultimately not more likely to retain their homes. It should be noted that the Massachusetts law was drastically different than the CFPLs as the aforementioned right-to-cure law did not increase the institutional foreclosure costs for lenders. Also, the housing bust in Massachusetts was substantially less severe than that experienced in California. Below, we remove Massachusetts from our analysis to avoid any potential contamination of the control group, but our results are robust to the inclusion of Massachusetts as well.

Finally, during the Great Depression, several states enacted foreclosure moratoriums in an attempt to prevent widespread farm foreclosures. In an analysis of these laws, Alston (1984) and Rucker and Alston (1987) find that these moratoriums did provide relief for some borrowers, but increased the costs on private creditors and thus made it more difficult for borrowers that were exposed to the moratoriums to obtain credit in later periods.[33]

3 Hypothesis

The CFPLs may have impacted housing markets through two channels. First, to encourage lenders to modify mortgages in default, the CFPLs increased the required stipulations and

[31] See Agarwal et. al (2015), Tracy and Wright (2016) and the references therein.

[32] "BofA Give-Away Has Few Mortgage Takers Among Homeowners: Mortgages." *Bloomberg News*. July 12, 2012.

[33] See Wheelock (2008) for a summary.

timeframe of the foreclosure process. These changes imposed notable incremental costs on lenders, and may have reduced the transition rate from delinquency to foreclosure.[34] Indeed, Mian et al. (2015) find that states with a more costly and lengthy judicial foreclosure protocol had dramatically lower foreclosure rates and higher house prices during the 2000s housing crisis. Thus, the findings from Mian et al. (2015) suggest that the more lengthy and costly foreclosure process created by the CFPLs may have reduced the flow of homes into foreclosure. From there, further results in Mian et al. (2015) indicate that a reduction in the rate at which foreclosures hit the market can increase house prices. As noted above, the economic justification for foreclosure modification and foreclosure abeyance can be traced to foreclosure externalities or theories of foreclosure induced fire sales. Second, the overriding intent of the CFPLs was to encourage the modification of mortgages for homeowners facing default. Hence, the CFPLs aimed to provide debt relief to mitigate excessive foreclosures. As has been widely noted in the literature, foreclosures result in losses for both borrowers and lenders and may also produce negative externalities for homeowners.[35] Together, these channels form a key testable hypothesis: CFPL foreclosure costs and related provision of mortgage modification programs caused (1) a decrease in the portion of mortgages entering default; and (2) a subsequent firming in house prices.

While the aforementioned research implies that the CFPLs should have had a beneficial impact on housing, leading policymakers have suggested otherwise. For example Larry Summers, who was the Director of the National Economic Council during the Great Recession, stated that the US Government elected not to increase the duration of the foreclosure process as such policies would have simply delayed foreclosures until a later period.[36] Note, however, that Summers did not discuss policies that would have increased the pecuniary costs associated with the foreclosure process.

Additionally, the CFPLs may also have had important implications for the real economy. If the CFPLs led to a large increase in housing wealth (as we document below), then an increase in consumption would follow as long as the propensity to consume from housing wealth is greater than zero. There is a large literature that attempts to estimate the propensity to

[34] Ciochetti (1997) and Pence (2006).

[35] See Bolton and Rosenthal (2002), Piskorski, Seru, and Tchistyi (2011), Campbell, Giglio, and Pithak (2011), Hartely (2014), and Anenburg and Kung (2014).

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

consume out of housing wealth, but little policy consensus on variation in that parameter over the economic cycle and in the wake of crisis-era mortgage modification. For example, Geithner (2014) contends that the MPC out of housing wealth from a mortgage modification program during the Great Recession would have been only 0.01 to 0.02 and thus that such a program would not have led to notable changes in the real economy. In marked contrast, Bostic et al. (2009) and the references therein contend that the MPC out of housing wealth is between 0.05 and 0.10. Our unique policy experiment allows us to assess the effects of mortgage modification on the real economy and calculate the MPC out of housing wealth during a severe economic recession.

4 Data

We undertake analyses of the effects of the CFPLs on housing and related markets using data at several different levels of geography including at the state, county, and zip code levels. More aggregated data, for example at the state-level, allows us to consider a wider range of variables and may also be more of interest to policymakers who seek to assess the broad effects of the policy. Disaggregated data is also advantageous given the breadth of California and large heterogeneity across local California housing markets. For instance in Los Angeles, there are 345 different zip codes that vary notably in both average house prices and income. More local data thus allows differing local effects of the CFPL policies, control for local housing and economic conditions, and yields a larger the number of cross-sectional observations. We discuss data at different levels of geography in turn. See appendix C for a complete data description.

State Level Data: This dataset includes prime, subprime, and total foreclosure starts (NOD, the first step in the foreclosure process) from the Mortgage Bankers Association (quarterly); the internet search query based Housing Distress Index (HDI) of Chauvet et al. (2014, henceforth CGL); the log first difference of house prices from Zillow (monthly) and the FHFA (quarterly); seasonally adjusted auto sales per 1000 people from Polk (quarterly); housing starts per capita (quarterly); non-farm payrolls per capita (quarterly); personal income per capita (quarterly); population density in 2005; the Saiz (2010) housing market supply elasticity; house price growth during the pre-treatment period (2004Q1-2008Q2; 2004M01-2008M06); housing return variance over the pre-treatment period (2004Q1-2008Q2;

2004M01-2008M06); house price growth one year prior to the treatment (2007Q2-2008Q2; 2007M06-2008M06); and housing returns one quarter prior to the treatment.[37] Additionally, the Zillow house price data are available for All Homes; the Top, Middle, and Bottom Tiers (thirds) of the housing market in terms of price. We use the returns on all of these Zillow housing proxies in our Synthetic Control Method (SCM) analysis. Data are available beginning in 2004Q1 (2004M01). The CFPL treatment period runs from 2008Q3-2010Q4 (2008M07-2010M12).

County Level Data: At the county level, we employ the monthly the Zillow house price data and auto sales data mentioned above, median household income (annual), the unemployment rate (annual), population density in 2005.[38] Also Shape Files for California counties used below are from the 2010 US Census.[39]

Zip Code Level Data (5 Digit): Data available at the zip code level include Zillow All Homes house price index, Income per household in 2007 from the IRS Statistics of Income, and the population density in 2007 using the population estimate from the IRS and land area estimates from the US Census Bureau.[40] Using the Zillow All Homes indices, we construct the same house price dataset used at the state level. The Shape Files used to determine the geo-locations of California zip codes are downloaded for 2013 data from the US Census Bureau.[41]

Zip Code level Data (3 Digit):

5 Econometric Methodology–Synthetic Control

To assess the impact of the CFPLs on key housing and macroeconomic outcomes, we apply the Synthetic Control methodology of Abadie, Diamond, and Hainmueller (2010) and Abadie and Gardeazabal (2003).[42] The Synthetic Control Method (SCM) implements comparative

[37]Auto sales are seasonally adjusted using the X-13 algorithm from the US Census Bureau. Housing starts, non-farm payrolls, and personal income are converted to per-capita terms using population data from the FRED database of the Federal Reserve Bank of St. Louis. Population Density and total land area used to compute the population density is from `https://www.census.gov/geo/reference/state-area.html`.

[38]Median household income and population by county are from the 1 Year ACS Community Survey, unemployment data is from the Bureau of Labor Statistics. County land areas are from 2010 US Census and were downloaded from the American Fact Finder website.

[39]`http://www.census.gov/geo/maps-data/data/tiger-line.html`.

[40]Estimates for the population and the number of households are also from the IRS Statistics of Income. Land area estimates are from 2010 US Census: `https://www.census.gov/geo/maps-data/data/zcta_rel_layout.html`.

[41]`https://www.census.gov/geo/maps-data/data/cbf/cbf_zcta.html`

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

case studies and thus estimates the causal effects of policy announcements on aggregate units. In essence, the SCM employs data-driven techniques to select an optimal control unit from a set of potential candidates not exposed to the treatment. In our state-level analysis, we use the SCM to develop a "Synthetic California," an optimal linear combination of other states, whose key economic aggregates can then be compared to the actual values from California. This process allows us to examine the causal impact of the CFPLs on crucial variables of interest including housing returns and auto sales.

We define a Synthetic Control as a linear combination of potential controls that approximates the most pertinent characteristics of the treated unit (Abadie, Diamond, and Hainmueller (2010)). Suppose that we observe $j = 1, \ldots, J+1$ units for $t = 1, \ldots, T$ time periods. 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 is the passage of the CFPLs in the state of California. Let the intervention occur 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 use the SCM to construct a reasonable approximation for this missing potential outcome.

To build the 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.[43] The advantage of this approach is that it generalizes the familiar differences-

[43]See Abadie, Diamond, and Hainmueller (2010) for the more general case where multiple pre-intervention

in-differences estimator as linear combinations of pre-intervention outcomes can be used to control for unobserved common factors that vary over time.[44]

In practice, typically there is no set of weights such that equations 1 and 2 hold exactly, so we follow Abadie, Diamond, and Hainmueller (2010, 2011) 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

$$||X_1 - X_0 W||_V = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)} \tag{3}$$

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 $(U_j', \bar{Y}_j^1)'$, and V is an $(r+1) \times (r+1)$ symmetric and positive semi-definite matrix. V is chosen to minimize the mean square error of the Synthetic Control estimator (the expectation of $(Y_1 - Y_0 W^*)'(Y_1 - Y_0 W^*)$). 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.

6 Preliminary Analysis: A Sand States Case-Study

Prior to embarking on our SCM analysis, we first undertake a traditional differences-in-differences (diff-diff) case study analysis of the CFPLs using the Sand States–Arizona, California, Florida, and Nevada. Although choosing a comparison group within a case study diff-diff analysis is often difficult and "ad-hoc" (Peri and Yasenov (2015) and Card (1990)), the Sand States make a natural choice 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 lumped together in descriptions of the excess that

linear combinations are used.

[44] See Abadie, Diamond, and Hainmueller (2010, 2011) and Billmeier and Nannichi for more details.

transpired during 2000s housing boom.[45]

Figure 1 plots total, prime, and subprime foreclosure starts as a percentage of the corresponding number of outstanding loans; Zillow REO foreclosures per 10,000 people; the growth in housing distress (HDI); housing returns (FHFA and Zillow); and auto sales per 1000 people for the Sand Sates from 2004 through the end of 2013. In each plot, the path of the variable for California is the black-bold line, the other Sand States are the gray lines. In the figure, we denote the passage of SB-1137 in 2008Q3 (2008M07) with the long-dashed-red vertical line, the passage of the CFPA in 2009Q1 (2009M02) with the dash-dot-green vertical line, the implementation of the CFPA in 2009Q2 (2009M06) with dashed-blue vertical line, and the sunset date for the CFPA (the end of our policy analysis period) with the two-dashed pink vertical line in 2011Q1 (2011M01). The policy period of interest ranges from the announcement of SB 1137 in July 2008 through the sunset of the CFPA in January 2011. Yet we show the path of all variables through the end of 2013Q4 (2013M12) to determine if there is any reversal in the policy effects after the conclusion of the CFPA.

First, the Sand States generally yield an apt comparison group for California during the pre-treatment period (prior to the passage of SB-1137 in 2008Q3), especially for foreclosure starts, Zillow REO, the MDRI, foreclosures, and housing returns. Indeed, the pre-treatment foreclosure and housing return variables move in lockstep across the Sand States and the pre-treatment correlations in these variables are all near 1. Given the similarity of the Sand States prior to 2008Q3, Arizona, Florida, and Nevada provide an appropriate counterfactual for California during the CFPL treatment period. The one exception is California auto sales which are slightly lower during the pre-treatment period. Hence, the Sand States may not constitute an appropriate control group for this variables. The most immediate impacts of the policy are seen in foreclosure starts and REO foreclosures. Foreclosure starts moved in unison across the Sand States until SB-1137 went into effect in 2008Q3 (long-dashed-red vertical line). California foreclosure starts then fell notably, suggesting that the increased costs created by SB-1137 initially muted the ascension of foreclosure starts in California. From there, further increases in California foreclosure starts were limited by the passage and implementation of the CFPA. While there was a decline in both prime and subprime foreclosure starts, the

[45] For example, in this 2009 description of the economy by the FDIC, Arizona, California, Florida, and Nevada are lumped together as sand states. https://www.fdic.gov/bank/analytical/quarterly/2009_vol3_1/anatomyperfecthousing.html.

drop was especially large for subprime foreclosure starts after the introduction of SB-1137, indicating that this law had an outsized impact on the subprime market. In contrast, the implementation of the CFPA in June 2009 at the end of 2009Q2 corresponded to a more noticeable drop in prime foreclosure starts in 2009Q3. The bottom-left and top-middle plots in the figure also show a decline in REO foreclosures and the growth of Mortgage Default Risk (MDRI) fell with the passage of SB-1137 and then again as California passed and implemented the CFPA. With regard to prices, the middle and right columns of the figure show that both FHFA and Zillow California housing returns ticked upwards over the CFPL period with the most notable deviations from the other Sand States beginning in 2009Q1. In total, the path of the housing variables after 2008Q3 illustrates the comparable improvement in California housing market dynamics in the wake of the CFPL policies as foreclosure starts were markedly lower and returns were higher during the CFPL period. The CFPLs therefore appear to have led to a broad-based improvement in housing market conditions. Finally, we see little change in the path of auto sales during the CFPL treatment period.

Table 1 presents the path of each outcome variable across the Sand State during the CFPL treatment period (2008Q3-2010Q4). House prices and Mortgage Default Risk are presented as the change in logs over the treatment period; all other variables are the cumulative sum of the levels. The far-right column shows the diff-diff means estimate. The results in table 1 show that the introduction of the CFPLs coincided with a dramatic relative improvement in the California housing market: During the CFPL treatment period we find a large relative reduction in the portion of homes that entered the foreclosure process via a foreclosure start (10.14 percentage points) and a notable relative drop in the number of REO Foreclosures per 10,000 people (437.76). We also see similar declines in Mortgage Default Risk. Further, California state-wide house prices fell between just 20.19 (FHFA) and 22.03 (Zillow) percent during the CFPL period, while the smallest house price drop among the other Sand States was the 30.96 percent decline in Florida measured using the FHFA HPIs. The corresponding diff-diff means estimator for overall California returns relative to the other Sand States is thus large in magnitude and ranges from 16.83 to 19.35 percentage points. These effects are magnified for Middle and Bottom Tier homes. Despite this overall differential improvement in the housing market, we find little change in auto sales per capita, with a diff-diff means estimate of -6.41.

We also conduct the diff-diff analysis at the county-level, which increases the number of cross-sectional observations and allows us to control for local housing and macroeconomic conditions during the pre-treatment period. The results are in table 2 and show the average county-level REO foreclosures and house price growth within each state over the CFPL period; the corresponding standard deviations are in parentheses. The bottom panel displays the results for auto sales. California experienced lower REO foreclosures higher house prices compared to the other Sand States, in line with a relative improvement following the introduction of the CFPLs. Yet the standard deviations of house price growth within each state are large in magnitude, highlighting the large geographical heterogeneity even within states. The far-right column of the table shows the diff-diff estimates, where we regress the outcome variable at the county-level (REO foreclosures or house price growth) on an indicator for California using population weights and controlling for pre-treatment house price growth (2004Q1-2008Q2), house price growth one year prior to the treatment (2007Q2-2008Q2), the pre-treatment housing return variance, the unemployment rate in 2007, median income in 2007, and the population density in 2005. White standard errors are in parentheses. The diff-diff estimates imply that California counties performed significantly better than their Sand State counterparts in terms of both foreclosures and house prices. For example, overall house price growth was 9.80 percentage points higher for the counties in California. This estimate is smaller than the above state-level means diff-diff result. We also find substantial variation across housing market tiers using the county-level data. Indeed, the diff-diff estimate for Bottom Tier homes is 24.88 percentage points, while that for the most expensive home in the Top Tier is only 4.34 percentage points. Last, the bottom panel shows that auto sales in California counties were lower than in the other Sand States. However, as as noted above and seen in figure 1, the path of auto sales in Arizona, Florida, and Nevada do not closely match California during the pre-treatment period.

Table 3 shows the results from a zip code level diff-diff analysis using just the Sand States. The format of 3 is identical to that of table 2. The far right column shows the diff-diff estimate where controls include pre-treatment housing return variance, pre-treatment house price growth, house price growth one year prior to the treatment, household income, and population density in 2007. The regression is estimated using population weights. The results indicate that the CFPA had a notable impact on California housing markets: Average house price growth across California zip codes was substantially higher than in the other Sand States and the diff-diff estimate is statistically significant and large in magnitude at 8.50 percentage points.

Altogether, this case study indicates that the CFPLs attenuated the decline in the California housing market, resulting in a positive relative effect compared to the other Sand States. Yet our choice of a comparison group was arbitrary and based on widely held convictions of housing market dynamics across states. Below we use all available data to create Synthetic Control units. This data-driven approach allows us to build a counterfactual based on comparative characteristics across the treatment and control groups and assess the causal impact of the CFPL policies.

7 Main Results – Synthetic Control

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.

7.1 State-Level Results

At the state-level, outcome variables of interest include total, prime, and subprime foreclosure starts (quarterly); Zillow REO Foreclosures (monthly); Mortgage Default Risk (monthly, MDRI); housing returns from both the FHFA (quarterly) and Zillow (monthly); and auto sales per 1000 people (quarterly). For each of these variables, we search for a Synthetic match using the following predictors: Housing returns, auto sales, foreclosure starts, nonfarm payrolls, personal household income, housing starts, the housing return variance and house price growth over the pre-treatment period (2004Q1-2008Q2; 2004M01-2008M06), the house price growth 1 year prior to the treatment (2007Q2-2008Q2; 2007M06-2008M06), the house price returns the quarter before the treatment, the Saiz (2010) housing supply elasticity proxy, and the population density in 2005.[46] Here the pre-treatment period is from 2004Q1

[46] When the HDI represents the outcome variable, we also include the HDI in the set of predictors as recommended by Abadie et al. (2011). When foreclosure starts or auto sales is the outcome variable, housing returns are measured using the FHFA quarterly indices. For the HDI, housing returns are measured using the monthly Zillow All Homes house price returns. In all other cases, the housing return proxy matches the outcome variable.

to 2008Q2 (2004M01 to 2008M06) and the treatment period runs from 2008Q3 to 2010Q4 (2008M12).

The results are in tables 4 and 5 and in figure 2. To start, table 4 displays the contribution of each state to California's Synthetic Control for each outcome variable. Here, we list the number of states in the donor pool (which is based on data availability) and the SCM weight applied to each state. For brevity, only states with positive weight are listed. The results generally match our expectations. First, as seen in the top panel, when foreclosure starts, REO foreclosures, or the MDRI represent the outcome variable, California's Synthetic is comprised largely of Nevada and Florida, two states that experienced substantial house price busts during the recent crisis. Results in the second panel indicate that Nevada and Florida constitute California's Synthetic for FHFA housing returns. Using the Zillow returns, we see that California's Synthetic makeup is similar across different housing tiers. For example, California's Synthetic for the Zillow All Homes house price returns is built largely from Nevada and Florida as are the Synthetics for California Middle and Bottom Tier returns, with Virginia, Minnesota, and Michigan also receiving weight. Last, the Synthetic makeup when auto sales represents the outcome variable consists largely of Nevada, Kentucky, and Minnesota. Overall, these matches are congruent with our expectations and suggest that California is best approximated by other housing bust states such as Nevada and Florida.

Graphically, we can see the accuracy of the Synthetic matches by comparing the left and right panel figure 2 during the pre-treatment period to the left of the long-dashed-red vertical line that represents the passage of SB-1137. Here, for each outcome variable we plot the path of the of California versus the sample average in the left panel and California versus its Synthetic Control in the right panel. The vertical lines are the same events highlighted in figure 1. As seen across the figures, during the pre-treatment period the path of the California and the Synthetic Unit move in lockstep, while the sample average deviates notably from California in nearly every plot in the left panel of figure. This is implies that the Synthetic Control creates suitable match for California, especially compared to the sample average.

The Synthetic Control estimation results are in table 5 and figure 2. In table 5, we show the pre-treatment root mean-squared forecast error (RMSFE) and the change in the path of the outcome variable from 2008Q3 - 2010Q4 (2008M07 - 2010M12), from when the SB-1137 was passed to the sunset date for the CFPA, for both California and its Synthetic Control.

House prices and Housing Distress 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 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 in the far right column of the table. 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 of all estimated placebo effects.

First, the pre-treatment RMSFEs between California and its Synthetic Control, in the left column of table 5, 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 standard deviations. As noted above, the quality of the match during the pre-treatment period is also highlighted by figure 2 as California and the Synthetic Control move in tandem during the pre-treatment period. The top panel in table 5 presents the SCM results for foreclosure starts, REO foreclosures, and the MDRI. During the CFPL period, 15.96 percent of California mortgages entered foreclosure process (foreclosure start), compared to 24.46 percent for the Synthetic Control. The Gap between these estimates, the treatment effect from the CFPLs, is -8.50. Hence, 8.50 percent fewer mortgage loans entered default from 2008Q3-2010Q4, implying that the CFPLs lowered the portion of homes that entered foreclosure by one-third. 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 one-third (20.32/66.03). The far-right column of table 5 shows the Gap estimate is in the 3rd percentile of all placebo effects, indicating the effect of the CFPL treatment effect was rare and large in magnitude. Graphically, the causal impact of the CFPLs on foreclosure starts is displayed in the top three plots in the right column of figure 2. After the introduction of the CFPLs, foreclosure starts fell markedly compared to the Synthetic counterfactual, indicating that CFPLs dramatically reduced the incidence of default in California. Notice also that the path of California foreclosure starts did not spike and never crossed the path of the Synthetic after the conclusion of the policy, indicating that CFPLs did not simply delay foreclosure starts until a later period or prolong the crisis. Instead the CFPLs appeared to have a permanent positive effect on California housing markets. This result is at odds with the contentions of Larry Summers and Tim Geithner who suggested that increased foreclosure durations would simply delay foreclosures until a later period.[47] The fourth set of plots in figure 2 presents the treatment effects of the CFPLs on Zillow REO foreclosures. Clearly, there was a large drop in foreclosures due to the introduction of the CFPLs. The estimates in table 5 show that the CFPL treatment prevented 204 REO foreclosures per 10,000 people in California during the treatment period. This translates into a 25 percent reduction in REO foreclosures. The next set of plots shows the HDI housing distress measure of CGL. In line with our above results for foreclosures, the MDRI fell notably during the CFPA period, indicating that household mortgage default risk dropped due to the introduction of the CFPLs. Indeed, the Synthetic Control estimates for the MDRI in the fifth row of table 5, indicate that the CFPLs lowered the MDRI in California by approximately 24 percent over the treatment period. The corresponding Gap Percentile for the MDRI is 8.00 and thus the reduction in Housing Distress was large in magnitude.

In the second panel of table 5, we show the estimation output when FHFA housing returns are the outcome variable. During the CFPL period, California FHFA house prices fell 20.19 percent, while those for the Synthetic plunged 37.96 percent. The corresponding Gap and the estimated treatment effect of the CFPLs for FHFA house price returns is 17.77 percentage points. This effect is large in magnitude and implies that the CFPLs nearly halved the fall California house prices from 2008Q3-2010Q4. The Gap Percentile of the estimated treatment effect, relative to all placebo effects, is 100, supporting a causal interpretation of the results. Graphically, the right plot in the fourth row of figure 2 shows that following the implementation of the CFPLs that California housing returns jumped from nearly -10 percent per quarter just prior to the treatment to 0 percent in late 2009.

The Synthetic Control estimates using the Zillow All Homes house price indices show that during the CFPL period that California Zillow house prices fell 22.03 percent, those for the Synthetic dropped 36.38 percent, and hence the Gap estimate is 14.35 percentage points. This effect is rare and large in magnitude compared to all placebo effects with a Gap Percentile of 100. As the median California house price in June 2009 was $413,200, the

[47]Note however that neither Summers nor Geithner considered policies that would increase foreclosure costs. "Lawrence Summers on 'House of Debt' ". *Financial Times.* June 6, 2014. "Geithner Calls Foreclosure Moratorium 'Very Damaging' ". *Bloomberg News.* October 10, 2010.

14.35 percent relative increase in Zillow All Homes house prices translated into an increase of California housing wealth of approximately \$724.3 billion dollars.[48] The wealth increase for owner occupied housing units was \$356.3 billion.[49]

Graphically, in the seventh row of figure 2, we can see the relative increase in Zillow All Homes housing returns. Over the CFPL period (2008M07-2010M12), California housing returns were noticeably higher than those of the Synthetic before falling back in line with those of the counterfactual in 2011. A comparison of these Zillow All Homes results with our above FHFA SCM findings shows that the Gap estimates are nearly identical across the two measures, highlighting the robustness of our results to different house price index methodologies. The FHFA and Zillow SCM plots also show that the California returns did not fall below their Synthetic counterparts at any point during the CFPL period or through the end of 2012. This suggests that the CFPLs gains were not temporary and that the CFPL did not simply delay California the crisis until a later period. Altogether, the CFPLs reduced the slide in housing returns and attenuated the negative effects of the 2000s housing crisis.

Table 5 also shows that the CFPL relative house price gains extended to all housing market tiers. Further, the Gap Percentiles across the Zillow housing market tiers (except for Top Tier homes) are all large and therefore support a causal interpretation of the results.

Last, the bottom panel of table 5 displays the SCM estimation output for auto sales. Graphically, the results are in the final row of figure 2. The results show that California and its Synthetic move in unison during the pre-treatment period with a SCM RMSFE of just 0.17. Then after the implementation of the CFPLs, there is no noticeable deviation in the path of California relative to its Synthetic, suggesting that the CFPLs had a limited impact on California aggregate auto sales.

7.2 County-Level Results

Next, we measure the local effects of the CFPLs on REO foreclosures, housing returns, and auto sales by building a Synthetic Control unit for each California county.[50] The path of the outcome variable for each California county is compared to its corresponding Synthetic, yield-

[48] House price data are from Zillow. The total number of California housing units from the 2008 1-Year ACS Community Survey Table S1101 is 12,176,760. \$413,200*(0.1435)*12,214,891 $\approx$ \$724.3 billion.

[49] The total number of California owner-occupied housing units from the 2008 1-Year ACS Community Survey Table B25081 is 6,946,432. \$359,000*(0.1435)*6,910,054 $\approx$ \$356.0 billion.

[50] To ease the computational burden of the Synthetic Control approach, for the placebo experiments we use the average weights across California counties calculated in equation 3.

ing the Gap estimate and thus the causal impact of the policy for that county. Our dataset includes 39 counties across California; the donor pool consists of 537 other counties outside of California. 21 counties in California are excluded due to missing data.[51] For each county, we have Zillow REO foreclosures (monthly) or house price returns (All Homes, Top Tier, Middle Tier, and Bottom Tier; monthly), auto sales (quarterly), the unemployment rate (annual), median household income (annual), the population density in 2005, the pre-treatment house price growth (2004M01-2008M06), the pre-treatment house growth 1 year prior to the treatment (2007M06-2008M06), and pre-treatment housing return variance (2004M01-2008M06), and the housing returns one quarter prior to the treatment. These proxies capture key housing market dynamics and broad macroeconomic effects. As above, the outcome variables of interest are REO foreclosures, housing returns, and auto sales; all other variables are used during the pre-treatment period to build the Synthetic Control. Appendix C contains a complete description of the data along with a list of the California counties used in our sample and their corresponding FIPs codes.

Table 6 shows the complete county-level SCM estimation results including the RMSFEs, the estimated Gap in REO foreclosures, house price growth, or auto sales during the CFPL period (2008M07-2010M12; 2008Q3-2010Q4), and the percentile of the Gap relative to all estimated placebo effects. A summary of these results is in table 7 and figure 4. First, table 7 shows the population weighted and housing wealth weighted averages of the SCM RMSFEs and Gap estimates across California counties. The weighted average RMSFEs over the pre-treatment period are small in magnitude, especially compared to the average pre-treatment standard deviations. Thus, the Synthetic Control units thus yield appropriate matches for California counties over the pre-treatment period on average. Yet a closer inspection of table 6 indicates that there are California counties with poor Synthetic matches. For the Zillow REO foreclosures, for example, these counties, including San Joaquin and Stanislaus, are sparsely populated and have large agricultural sectors. Housing markets variables for these counties are also most likely less precisely estimated. Table 7 also shows the population or housing wealth weighted Gap estimates. When Zillow REO foreclosures are the outcome variable, the results imply that there were 92 fewer foreclosures per 10,000 people due to the introduction

[51] More counties have missing data when the outcome variable is Zillow REO foreclosures or Zillow returns for Middle and Bottom Tier Homes.

of the CFPLs. This implies that the CFPLs saved 330,000 homeowners from REO foreclosure during the CFPL treatment period.[52] Note that this estimate is more conservative than our above results that employed aggregated state-level data. With regard to prices, we find that the average Gap estimates are positive, but also that these estimates are generally smaller than the state-level results found above and our zip code results discussed below. Indeed, these county level estimates are the most conservative in our paper and, for house prices, range from 4.44 to 5.07 percentage points. Yet even these results suggest that the CFPLs created substantial California housing wealth, equivalent to $250 billion dollars.[53] The far-right column of table 7 displays the average county-level results for auto sales. On average, the Synthetic units yield appropriate matches for California counties when auto sales represent the outcome variable but that the CFPLs had a negligible effect on auto sales. Indeed, the Gap estimate of 8 vehicles per 1000 similar to our above state-level results

Figure 4 presents the results graphically in California choropleth plots. Here, red, grey, and blue represent negative, near zero, and positive Gap estimates, respectively. The colors in the plot become brighter as the Gap increases in magnitude. Counties that are colored white have no data and are excluded from our sample. We print the names of counties whose Gap estimate is below the 15th percentile for REO foreclosures (above the 85th percentile for house prices or auto sales) on the choropleths where one, two, and three asterisks printed next to each county name represents a Gap below the 5, 10, or 15th (above the 85, 90, or 95th) percentiles relative to all estimated placebo effects, respectively. The results in the choropleths indicate that the CFPLs were most efficacious in Southern California including in the hard hit areas of Los Angeles, Riverside, and San Bernardino. For example, in Los Angeles County, a quintessential housing boom and bust county, there were 175 fewer REO foreclosures and house prices were 15 percentage points higher than the counterfactual due to the introduction of the CFPLs, estimates that are large compared to the estimated placebo effects. As there were 3,181,903 housing units in Los Angeles County in 2007 with an median price $456,000, the 15 percent CFPL house price increase corresponds to an increase in Los Angeles County housing wealth of $215 billion–implying the CFPL housing gains largely

[52] California's population in 2008 was 36.6 million. Thus $92/10,000 * 36,600,000 \approx 335,000$.

[53] Using 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 2007M06 according to Zillow was $413,000. Thus, $413,200*12,200,672*0.05 ≈ $241 billion.

manifested in the hard hit Los Angeles area.[54]

In San Bernardino and Riverside Counties, two of the most hard-hit counties during the crisis in Inland Southern California, there was variation in the CFPL house price gains across house price tiers. For example, Top Tier and Middle homes in San Bernardino County experienced notable house price gains, while Bottom Tier homes did not. Similarly, prices for Top and Bottom Tier homes increased in Riverside following the CFPLs, but there was no corresponding gain for Middle Tier Homes. This variation highlights the heterogeneous impact of the CFPL policy across homes in Inland Southern California. Further, note that counties such as Riverside and San Bernardino include large, sparsely populated areas and thus the county-level house price indices incorporate a broad range of both urban and rural areas.

The choropleths in figure 4 also indicate that there are some counties where the Gap estimates for housing returns are negative and large in magnitude. However, these counties have small populations, are sparsely populated, and often have large agricultural sectors. For example, the Gap for Lake County, in central-northern California, was negative and large. But the population for Lake county is just 64,665.

Finally, the choropleth for auto sales shows that the CFPA did not have a positive impact on durable consumption as measured by auto sales. This result persists even for Los Angeles, which experienced substantial house price growth across all housing market tiers.

Figure 5 provides a closer look at the CFPL effects in Los Angeles County – an area that comprises over a quarter of California's population whose housing market benefited noticeably from the CFPL policies. The plots show that marked drop in REO foreclosures with the passage of SB-1137 and further dampening in the path of foreclosures with both the passage and implementation of the CFPA. The middle panel shows the these reductions in foreclosures translated into increases in house prices. Yet despite these beneficial housing market effects there was no change in the path of California auto sales, implying that the the CFPLs had a limited impact on the real economy.

[54]House prices are from the Zillow All Homes HPI in 2008M06. The number of households is from the 2007 ACS survey–table S1101. 3,181,903*(0.1298)*$456,000 $\approx$ $188 billion.

7.3 Zip Code Level House Price Returns

Last, we use the SCM to evaluate the impact of the CFPLs on zip code level house prices as measured by the Zillow All Homes indices. For ease of computation, we restrict the SCM donor pool to Arizona, Florida, and Nevada, the other Sand States that were shown to closely approximate California above in section 6. Thus, we construct a Synthetic match for each of California's 1196 zip codes using 1140 zip codes from Arizona, Florida, and Nevada.[55] The outcome variable of interest is Zillow All Homes returns and we build a Synthetic match for each California zip code using housing returns, the pre-treatment house price growth (2004M01-2008M06), the pre-treatment house growth 1 year prior to the treatment (2007M06-2008M06), and pre-treatment housing return variance (2004M01-2008M06), the housing returns one quarter prior to the treatment, Income per household in 2007, and the population density in 2007.

The results for the zip code level SCM estimates are in tables 8 and 9. First, table 8 shows the unweighted and population weighted averages of the SCM RMSFEs and Gap estimates for all California zip codes and for zip codes who's Gap Percentile is greater than 95, between 90 and 95, and between 85 and 90. Based on the permutation tests, the cutoffs for the 85, 90 and 95th percentiles of the Gap estimates are 10.87, 13.46, and 17.48 percentage points, respectively. The top row indicates that the population weighted average RMSFE is just 0.53, approximately just one-tenth of the annualized population weighted pre-treatment standard deviation. Therefore, that SCM produces apt matches for California zip code codes on average. The population weighted Gap estimate for house price growth during the CFPL period is large in magnitude at 9.52 percentage points. This zip code Gap estimate nearly directly splits the above county- and state-level estimated effects.

Further, nearly a quarter (279/1196) of all California zip codes had a Gap Percentile greater than 95, highlighting large economic impact of the CFPLs in many areas. The population weighted average Gap estimate for these zip codes is 23.79 percentage points; hence nearly a quarter of California zip codes experienced CFPL house price gains of 23.79 percent. There are 242 other zip codes with a Gap Percentile between 85 and 90 and therefore nearly

[55] To further ease the computational burden of the Synthetic Control Approach, we use the variable weights for equation 3 from California at the state-level in each zip code estimate for California and in each placebo experiment.

50 percent of California zip codes have a Gap percentile greater than 85 and a CFPL Gap greater than 10.17 percentage points. Finally, note that the population weighted average SCM RMSFEs for zip codes with large gap estimates are all small in magnitude.

Next, in table 9 we assess the heterogeneous geographic effects of the CFPLs and break down the zip code SCM Gap estimates by geographic region. We create the following broad geographic regions within California: Central California (Central), Inland Southern California (Inland), Los Angeles (LosAngeles), Northern California (Northern), San Diego (SanDiego), Southern Los Angeles (SouthLA), and Other.[56] Within the broad LA area, Inland includes cities such as Ontario, Riverside and San Bernardino; LosAngeles consists of Downtown LA and the surrounding areas; and SouthLA is made up of Anaheim, Newport Beach, and Irvine. Table 9 shows the portion of zip codes within each region with a Gap Percentile greater than 95, between 90 and 95, and between 85 and 95. The bottom row of the table displays the total number of zip codes in each region. Clearly, large CFPL effects are highly concentrated in the greater Los Angeles area. In Inland Southern California (Inland), for example, 45.10 percent of zip codes had a CFPL Gap Percentile greater than 95 and thus a Gap estimate greater than 17.48 percentage points. In total, 63.72 percent of Inland zip codes have a Gap Percentile greater than 85 and thus a Gap estimate larger than 10.87 percentage points. Similarly, over 70 percent of zip codes in LosAngeles and SouthLA have a Gap Percentile greater than 85. In marked contrast, the portion zip codes in all other California regions with a Gap percentile greater than 85 does not exceed 34.82 percent (Central).

7.4 Credit Constraints and Refinancing Volume

7.5 Other Transformations for Auto Sales

In our previous analysis, we used the number of auto sales per 1000 people as the outcome variable of interest and found no impact of the CFPLs on this proxy of durable consumption. We repeated this analysis using different transformations of auto sales including log, log first-

[56]We define the these regions as follows from South to North: SanDiego is east of where California State Route 54 meets California State Route 95 and south of south San Clemente, lat < 33.392089 & long > -116.942938; SouthLA 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; LosAngeles 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; Inland 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.

difference, growth from 2004Q1 and growth from 2008Q2. In none of these cases was there a deviation from our above findings.

8 Did the CFPLs Create Adverse Side Effects for New California Borrowers?

The passage of the CFPLs increased the costs of the foreclosure process for lenders and thus ex post, after the loans were originated, potentially reduced the value of their foreclosure option. As noted by Alston (1984), in his analysis of foreclosure moratoriums during the Great Depression, if the value of the foreclosure option declines, lenders may respond in two ways, both yielding potentially adverse side effects for new borrowers. First, lenders may increase the interest rate on new mortgages to compensate for the reduced value of the foreclosure option. For the CFPLs, this line of thinking would translate into fewer loans originated in California after the policy in equilibrium, *ceteris paribus*. Second, lenders may ration credit, especially in environments where raising interest rates is infeasible, and only lend to higher quality borrowers. Indeed, Alston notes that following the Depression that 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 prevented lenders from increasing interest rates in California following the recent housing crisis. On the other hand, 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 value foreclosure option for these banks was limited due to the depths of the crisis. If the value of the foreclosure option was in fact diminished, then CFPLs may have not altered banks' expectation of the value of the foreclosure option after the implementation of the policy. Finally, if the CFPLs aided depressed California housing markets (as documented above), then lenders may view the CFPLs favorably as excess foreclosure create dead weight losses for lenders (Bolten and Rosenthal (2002)) and higher house prices increase the value of repossessed homes. Along these lines, Bolten and Rosenthal (2002) develop a theoretical framework and show that moratoriums always increase efficiency ex post, following an adverse shock.

To determine the impact of the CFPLs on mortgage borrowers purchasing homes following the implementation of the policy, we employ the HDMA dataset. The results are in table

10. First, we use loan-level data to determine if 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 regression model where the left-hand-side variable is an indicator that takes a value of 1 if the prospective borrower was denied a mortgage and zero otherwise. The key right-hand-side variable is an indicator that takes a value of 1 if the home is in California and zero otherwise. Controls include the log of the loan amount and the 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; the 2010 Census zip code level population density; and the following factor variables: applicant race, applicant sex, the institution that purchased the loan after origination (e.g. government organizations, private organizations, etc), the lien status of the loan, and owner occupancy. For zip code variables, the Missouri Data Bridge was used to convert census tract to zip code. These data range from 2009 to 2014. We first restrict the dataset to Arizona, California, Florida, and Nevada (columns 1 and 3 of the table), 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 and 4), as these latter states that were less affected by and rebounded relatively quickly from the crisis. Columns 1 and 2 show the results from a linear probability model (OLS estimation) and columns 3 and 4 report the marginal effects from a probit model. In the table, heteroskedasticity robust standard errors are in parentheses and asterisks indicate a p-value that is less than 0.01. To assess the magnitude of the coefficient on the indicator for California, we also show the coefficient on house price growth in the year before the loan application was submitted. Overall, the results show that the probability of denial in the post-treatment period was lower in California. Specifically, column 1 suggests that California borrowers were 12.9 percent less likely to be denied a mortgage loan compared to similar borrowers in AZ, FL, and NV. Similarly, comparing California to CO, NY, and TX, yields a coefficient on the indicator for California of -11.6 percent. These estimates are both significant with p-values of approximately 0. The coefficients also appear to be economically meaningful as they are within an order of magnitude of the coefficient on the house price growth in the year before the application was submitted. Columns 3 and 4 repeat this analysis but report marginal effects from a probit model; the results are similar.

Next, we consider loan volume growth following the implementation of the policy. Specifically, we consider the loan growth at the zip code level, both in terms of the number of loans and dollar volume, for 2009 through 2014 relative to 2007.[57] Columns 3 through 6 of table 10 show the estimates where the key left-hand-side variable is either the growth in dollar volume or the growth in the number of loans originated for both the Sand State and alternative samples described above. 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; Census zip code population density in 2010; and Zillow house price growth for 2008-2009 (crisis), 2010-2011 (emergence from crisis), and 2012-2014 (post-crisis). For comparison purposes, table 10 also shows the coefficients on the house price growth variables. In total, the estimates in columns 3 - 6 show that loan volume growth, in terms of both dollars and the number of loans originated, was higher in California zip codes. For example in the Sand States sample using dollar volume growth in column 3, 11 percent more credit flowed to California zip codes from 2009 to 2014, relative to 2007. This estimate is significant with a p-value of approximately zero and large in magnitude. Similarly, the remaining coefficients on the California indicator in columns 4 - 6 are also all positive and significant at the 1 percent level, indicating that credit growth was higher for California zip codes in the post-CFPL period.

In total, the results in table 10 indicate that new California borrowers were not adversely affected by the CFPLs and thus imply that lenders did not view the CFPLs as an extra cost that reduced the value of the foreclosure option. Rather, evidence suggests that Californians were more likely to receive mortgage credit after the implementation of the CPFLs, in line with favorable lender expectations of California housing markets post-policy.

9 Why were CFPLs Effective in Buoying California Housing Markets?

The foregoing analysis shows that the CFPLs substantially improved the California housing market relative to counterfactual estimates. In this section we further examine why the CFPLs were efficacious. First, consider the argument by leading US crisis-era policymakers,

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

Summers and Geithner, that an increase in durations would simply prolong the crisis and delay foreclosures until a later date.[58] If these predictions regarding the CFPLs transpired and the California policies simply delayed foreclosures, then this would result in (1) mortgages lingering in delinquency or in the foreclosure process during the policy period; and (2) a spike in foreclosures after the end of the policy period. To examine point (1), we plot the SCM estimation results for the portion of loans that are seriously delinquent (loans that are 90 days or more delinquent or in the foreclosure process), 90 days delinquent, and 60 days delinquent in figure 6. The data are from the Mortgage Bankers' Association and available at the state level. In the left panel, we show the path plot of delinquencies in California versus those for the Synthetic Control. The right panel displays the Gap in delinquencies between California its Synthetic (black line) and the estimated Gap from all placebo experiments (grey lines). First, the top panel of figure 6, shows that the portion mortgages that are seriously delinquent (90 days or more delinquent or in the foreclosure process), fell noticeably following the implementation of the policy and never spiked after the end of the policy period. Hence, mortgages at serious risk of default in California did not linger in a state of delinquency of in the foreclosure following the introduction of the CFPLs, but instead the policy reduced the portion of mortgages that were seriously delinquent. Further, the middle and bottom panels of figure indicate that there was noticeable increase in the portion of the loans in 90 or 60 days delinquency after the implementation of the policy compared to placebo estimates, indicating that mortgages were not lingering at these levels of delinquency.

As noted above in point (2), if the CFPLs simply delayed foreclosures until a later date, then there should be a spike in foreclosures following the end of the policy. Yet, as seen in figure 2, neiter foreclosure starts nor REO foreclosures for California jump after the conclusion of the policy or rise through through the end of 2013. Rather, California foreclosures remained depressed, relative to their counterfactual, until the end of the sample period, implying that the policy effects were long-lasting.

6

[58]In public remarks, neither Giethner or Summers discussed discussed programs that would have increased the duration and costs of the foreclosure process.

10 Conclusion

This paper uses the Synthetic Control approach to assess the of the housing and broader economic effects of the CFPLs, a unique set of mortgage modification programs that increased the cost and duration of the foreclosure process in an attempt to encourage widespread mortgage modification. We find that the CFPLs significantly attenuated the decline of the California housing market, reducing the number of homes that entered default during the treatment period by 7.27 percentage points. The corresponding increase in housing wealth, using our most conservative estimates, was $241 billion–a 4.79 percent increase. A back of the envelope application of our estimates to the other Sand 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 other markets: 350,000 homes in Arizona, Florida, and Nevada would have avoided default and housing wealth in these states would have increased $97 billion.[59]

Despite the positive impact of the CFPLs on housing markets, we find no evidence that these policies had a positive effect on durable consumption as measured through auto sales. These results hold for California overall and for areas where the CFPLs were highly efficacious, such as Los Angeles County. Thus, the broader economic effects of policies that increase foreclosure costs to encourage mortgage modification may be limited.

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.

[59]The number of homes with a mortgage in each state is table B25081 of the 2007 1-Year ACS Community Survey. Adding up the number of homes with a mortgage in Arizona, Florida, and Nevada and multiplying by the treatment effect yields: (1,088,152 + 3,288,932 + 445,322)*0.0727 ≈ 350,000. 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 + 7,088,960*$189,500 + 954,067*$233,600)*0.0479 ≈ $97 billion.

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A Tables

Table 1: Sand States – Foreclosures, Housing Distress, Housing Returns, and Auto Sales During the CFPL Treatment Period

	CFPL Treatment Period (2008Q3-2010Q4)				
	AZ	CA	FL	NV	Diff-Diff
Forc Starts (% of All Loans)	22.75	15.96	24.31	31.25	-10.14
Prime Forc Starts (% of Prime Loans)	18.17	12.90	19.96	26.93	-8.79
Subprime Forc Starts (% of Subprime Loans)	57.94	45.71	50.91	66.36	-12.69
Zillow REO Forc per 10,000 people	961.76	640.26	NA	1194.28	-437.76
Growth in Mortgage Default Risk (%; MDRI)	-19.94	-58.02	-26.58	-14.23	-37.77
FHFA Returns	-36.53	-20.19	-30.96	-43.56	16.83
Zillow All Homes Returns	-38.91	-22.03	-33.39	-51.84	19.35
Zillow Top Tier Returns	-30.29	-9.14	-22.62	-38.51	21.33
Zillow Middle Tier Returns	-39.81	-16.19	-32.88	-53.76	25.95
Zillow Bottom Tier Returns	-60.62	-40.26	-53.48	-82.58	25.31
Auto Sales per 1000 people	59.64	60.82	84.98	57.08	-6.41

Notes: When foreclosure starts, FHFA returns, and auto sales represent the outcome variable (quarterly data), the CFPL treatment period is 2008Q3 to 2010Q4. For the MDRI and Zillow data, the treatment period runs from 2008M07 to 2010M12. The far-right column shows difference between the outcome variable for California relative to the average of AZ, FL, and NV.

Table 2: Sand State County-Level Diff-Diff Results – Foreclosures, Housing Returns, and Auto Sales during the CFPL Period

	CFPL Treatment Period (2008Q3-2010Q4)				
	AZ	CA	FL	NV	Diff-Diff
Zillow REO Forc per 10,000 people	689.58	642.00	NA	1013.91	-349.15*
	(477.17)	(338.10)	(NA)	(470.14)	(88.11)
Zillow All Homes HP Growth	-29.29	-25.35	-28.85	-48.92	9.80*
	(11.91)	(10.25)	(10.11)	(10.87)	(4.05)
Zillow Top Tier HP Growth	-25.94	-17.50	-22.69	-34.53	4.34
	(8.43)	(8.31)	(8.25)	(10.97)	(3.28)
Zillow Middle Tier HP Growth	-29.37	-25.39	-29.03	-49.44	10.79*
	(11.36)	(10.04)	(9.72)	(10.25)	(4.35)
Zillow Bottom Tier Returns	-37.25	-41.19	-44.40	-74.62	24.88*
	(20.47)	(13.73)	(17.71)	(18.59)	(6.48)
Auto Sales per 1000 people	53.42	52.48	82.75	53.37	-24.82*
	(6.35)	(13.10)	(16.89)	(11.67)	(6.96)

Notes: See the notes for table 1. This table shows county-level averages within each state where the standard deviations are in parentheses. The far-right column holds the diff-diff regression estimates over the Sand State counties controlling for pre-treatment house price growth (2004Q1-2008Q2), the house price growth one year prior to the treatment (2007Q2-2008Q2), the pre-treatment return variance (2004Q1-2008Q2), the unemployment rate in 2007, median income in 2007, and the population density in 2005. The regression is weighted by population and White standard errors are in parentheses. An asterisk represents diff-diff significance at the 5 percent level.

Table 3: Sand States Zip Code Level Diff-Diff Results – Housing Returns During the CFPL Period

	2008M7 - 2010M12 Sum				
	AZ	CA	FL	NV	Diff-Diff
Zillow All Homes	-35.76	-24.66	-34.24	-48.89	8.50*
	(18.48)	(17.17)	(18.58)	(21.68)	(0.77)

Notes: See the notes for table 2. The far-right column holds the diff-diff regression estimate over the Sand State zip codes controlling for pre-treatment house price growth (2004Q1-2008Q2), the house price growth one year prior to the treatment (2007Q2-2008Q2), the pre-treatment return variance (2004Q1-2008Q2), the household income in 2007, and the population density in 2007. The regression is weighted by population and White standard errors are in parentheses. An asterisk represents diff-diff significance at the 5 percent level.

Table 4: State-Level Synthetic Control Unit Weights

Outcome Variable	Controls	Synth Weights
Forc Starts (% of All Loans)	41	NV: 0.69; CT: 0.19; MD: 0.11; MN: 0.01
Prime Forc Starts (% of Prime Loans)	41	NV: 0.69; MD: 0.31
Subprime Forc Starts (% of Subprime Loans)	41	NV: 0.98; FL: 0.02
Zillow REO Forc per 10,000 people	20	NV: 0.54; MN: 0.40; CT: 0.02; OH: 0.01; NE: 0.01
Growth in Mortgage Default Risk (MDRI)	33	FL: 0.36; NV: 0.30; MI: 0.29; MN: 0.03; WA: 0.03
FHFA Returns	41	NV: 0.58; FL: 0.39; MI: 0.02
Zillow All Homes Returns	36	NV: 0.41; FL: 0.34; MI: 0.16; MN: 0.09
Zillow Top Tier Returns	38	VA: 0.37; FL: 0.34; MI: 0.17; TX: 0.12
Zillow Middle Tier Returns	36	NV: 0.42; MN: 0.32; FL: 0.25
Zillow Bottom Tier Returns	37	NV: 0.65; MN: 0.22; FL: 0.13
Auto Sales per 1000 people	41	NV: 0.64; KY: 0.16; MN: 0.12; MI: 0.07; NY: 0.01

Notes: The Synthetic Control unit weights. The left column lists the outcome variable, the middle column shows the number of available controls, and the right column shows Synthetic Control weights. Only states weights with positive weight are listed.

Table 5: State-Level Synthetic Control Estimation Results

		CFPL Treatment Period (2008Q3-2010Q4)			
Outcome Variable	RMSFE	California	Synthetic	Gap	Percentile
Forc Starts (% of All Loans)	0.01	15.96	24.46	-8.50***	2.38
Prime Forc Starts (% of Prime Loans)	0.00	12.90	20.70	-7.80***	2.38
Subprime Forc Starts (% of Subprime Loans)	0.03	45.71	66.03	-20.32***	2.78
Zillow REO Forc per 10,000 people	2.45	640.26	844.71	-204.44**	5.26
Growth in Mortgage Default Risk (%; MDRI)	0.01	-58.02	-18.51	-23.86**	8.00
FHFA Returns	0.40	-20.19	-37.96	17.77***	100.00
Zillow All Homes Returns	0.03	-22.03	-36.38	14.35***	100.00
Zillow Top Tier Returns	0.14	-9.14	-12.46	3.32	75.00
Zillow Middle Tier Returns	0.03	-16.19	-35.39	19.19***	100.00
Zillow Bottom Tier Returns	0.06	-40.26	-64.27	24.01***	100.00
Auto Sales per 1000 people	0.17	60.82	60.43	0.39	65.79

Notes: State-level estimates from the Synthetic Control procedure. 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 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 far right column shows the percentile of the Gap estimate relative to all placebo effects. One, two, and three asterisks indicates that the Gap estimate for the treated unit is the greater (lower) than the 85, 90, and 95th (5, 10, and 15th) percentiles of all estimated placebo effects.

Table 6: County-Level Synthetic Control Estimates

	Zillow Foreclosures			Zillow All Homes			Zillow Top Tier			Zillow Middle Tier		
County	RMSFE	Gap	Percentile	RMSFE	Gap	Percentile	RMSFE	Gap	Percentile	RMSFE	Gap	Percentile
Alameda6001	0.56	126.43	94.12	0.05	11.16**	91.73	0.07	-0.63	36.79	0.11	0.64	41.70
Butte6007	NA	NA	NA	0.38	12.21**	94.17	0.20	3.18	63.77	0.23	15.22***	98.46
ContraCosta6013	1.65	-218.07***	4.58	0.04	-10.65	4.70	0.07	0.31	44.53	0.07	-3.04	19.88
ElDorado6017	NA	NA	NA	0.08	-12.14	3.95	0.10	-8.13	7.17	0.13	-9.10	4.83
Fresno6019	1.01	-178.63**	6.54	0.18	-0.19	39.10	0.18	2.61	58.87	0.19	-1.55	28.19
Humboldt6023	NA	NA	NA	0.42	-6.23	11.84	0.56	-9.91	4.72	0.39	-4.48	13.71
Imperial6025	NA	NA	NA	0.69	-21.87	1.13	0.29	-13.31	2.08	0.21	-17.75	0.97
Kern6029	1.40	-494.81***	0.65	0.24	-2.29	26.32	0.21	0.49	46.04	0.26	-4.90	12.36
Kings6031	0.74	-73.47	24.18	0.27	4.92	71.24	0.47	10.58**	93.02	0.30	4.43	68.73
Lake6033	1.31	-15.70	48.37	0.56	-4.80	15.23	0.88	-23.87	0.38	0.49	-8.74	4.83
LosAngeles6037	1.33	-174.91**	6.54	0.20	14.83***	96.24	0.14	6.78	81.51	0.18	8.95*	88.03
Madera6039	NA	NA	NA	0.76	-1.47	30.45	0.70	-3.72	18.87	0.73	-3.07	19.88
Marin6041	NA	NA	NA	0.08	-1.81	29.14	0.18	-3.93	18.68	0.09	-9.49	4.63
Mendocino6045	NA	NA	NA	1.92	-0.37	37.59	0.59	7.41	84.34	1.56	2.12	53.67
Monterey6053	NA	NA	NA	0.66	-0.42	36.47	0.47	9.52**	90.94	0.84	7.23	82.82
Napa6055	NA	NA	NA	0.20	-7.54	8.27	0.14	-1.66	31.51	0.17	-10.14	4.25
Nevada6057	NA	NA	NA	0.10	6.64	77.82	0.16	-3.45	20.00	0.11	0.29	39.77
Orange6059	0.52	-103.65*	11.76	0.32	7.10	80.26	0.28	-1.72	31.32	0.31	5.70	76.45
Placer6061	NA	NA	NA	0.22	-10.83	4.70	0.10	-7.31	8.49	0.11	-7.56	6.95
Riverside6065	5.22	154.07	95.42	0.36	-3.95	17.86	0.19	2.81	60.57	0.29	-7.26	7.92
Sacramento6067	5.31	55.49	86.93	0.15	-0.76	35.15	0.12	2.90	61.13	0.10	0.66	41.89
SanBernardino6071	2.75	-256.68***	1.31	0.27	-1.42	30.64	0.09	13.51***	96.42	0.08	11.66**	94.40
SanDiego6073	2.72	-156.39**	6.54	0.33	2.94	59.77	0.31	3.33	64.72	0.38	6.13	77.80
SanFrancisco6075	NA	NA	NA	0.37	6.39	76.69	0.30	-0.95	35.66	0.26	-0.76	33.59
SanJoaquin6077	36.09	533.37	100.00	0.24	1.38	50.75	0.43	-7.00	8.87	0.31	-4.70	13.32
SanLuisObispo6079	NA	NA	NA	0.50	-20.49	1.50	0.35	2.02	56.60	0.30	-15.34	1.16
SanMateo6081	0.18	4.52	62.09	0.12	-0.33	37.97	0.19	-2.40	26.23	0.16	-9.57	4.63
SantaBarbara6083	NA	NA	NA	0.30	4.16	66.35	0.33	3.33	64.72	0.31	6.32	78.57
SantaClara6085	2.38	-2.50	54.25	0.04	-1.17	32.71	0.08	-0.65	36.42	0.06	2.63	57.34
SantaCruz6087	NA	NA	NA	0.26	14.31***	95.68	0.16	-6.21	10.57	0.28	11.06**	92.86
Shasta6089	2.46	7.08	63.40	0.49	-11.00	4.70	0.19	4.40	72.08	0.29	-3.56	17.18
Solano6095	1.57	-135.21**	8.50	0.21	-12.61	3.76	0.12	-9.84	4.91	0.15	-17.18	0.97
Sonoma6097	1.19	-252.64***	1.31	0.18	3.00	60.53	0.14	0.39	45.09	0.21	-1.40	29.34
Stanislaus6099	9.80	492.65	99.35	0.19	-4.64	15.60	0.25	-15.18	1.32	0.19	-7.44	7.14
Sutter6101	NA	NA	NA	0.77	-13.48	2.82	0.68	-12.82	2.08	0.70	-6.25	9.27
Tulare6107	0.23	-123.21**	9.80	0.45	9.79*	88.53	0.31	-0.53	37.92	0.34	10.80**	92.86
Ventura6111	0.62	-83.67	21.57	0.23	2.31	57.71	0.28	5.85	78.49	0.23	4.71	71.04
Yolo6113	NA	NA	NA	1.25	-6.03	12.41	0.51	9.61**	91.32	0.85	-7.27	7.72
Yuba6115	NA	NA	NA	1.10	5.95	75.00	0.80	-1.52	31.70	0.62	3.87	65.44

Notes: County-level estimates from the Synthetic Control procedure where outcome variables are returns on the Zillow All Homes, Top Tier, Middle Tier, Bottom Tier, and Condominium House price indices and auto sales per 1000 people. Within each panel, we show the pre-treatment RMSFE, the Gap Estimate, and the Gap Percentile relative to all placebo effects. The FIPS code for each county is appended to the county name.

Table 6 Continued

	Zillow Bottom Tier			Auto Sales		
County	RMSFE	Gap	Percentile	RMSFE	Gap	Percentile
Alameda6001	0.10	-6.55	17.05	0.20	-1.08	42.64
Butte6007	0.26	-9.07	10.69	0.12	-2.91	34.03
ContraCosta6013	0.11	-12.78	5.85	0.27	-9.53	9.94
ElDorado6017	0.13	-11.54	7.63	0.41	-18.42	1.15
Fresno6019	0.18	-7.46	13.99	0.15	-5.67	21.22
Humboldt6023	0.68	0.02	42.24	0.11	-2.07	37.86
Imperial6025	0.34	-17.48	3.82	0.40	-13.24	5.16
Kern6029	0.24	-13.11	5.60	0.52	-5.51	21.80
Kings6031	0.40	3.32	57.00	0.55	-9.31	10.71
Lake6033	0.40	-19.31	3.31	0.14	-9.12	11.28
LosAngeles6037	0.08	31.71***	99.75	0.13	-2.32	36.71
Madera6039	0.87	-2.20	28.24	0.43	-12.49	5.93
Marin6041	0.08	-9.95	9.67	0.33	-7.36	14.34
Mendocino6045	NA	NA	NA	0.25	-15.72	2.49
Monterey6053	0.32	-18.83	3.31	0.24	-13.78	4.59
Napa6055	0.11	-5.32	20.61	0.32	-17.55	1.34
Nevada6057	0.11	19.71***	97.20	0.27	-1.73	39.39
Orange6059	0.45	-2.47	27.74	0.37	-11.51	6.88
Placer6061	0.26	-3.17	26.21	0.37	-20.30	1.15
Riverside6065	0.07	15.68**	94.66	0.49	-20.08	1.15
Sacramento6067	0.16	-13.91	5.34	0.52	-13.47	5.16
SanBernardino6071	0.23	-5.15	20.87	0.35	-15.59	2.49
SanDiego6073	0.35	7.91	77.86	0.55	-5.72	20.84
SanFrancisco6075	0.65	-12.20	6.62	0.13	-8.74	12.24
SanJoaquin6077	0.36	-3.51	24.68	0.52	-10.74	8.03
SanLuisObispo6079	0.11	2.10	51.65	1.00	-18.18	1.15
SanMateo6081	0.14	-7.79	11.96	0.21	-7.81	13.77
SantaBarbara6083	0.30	9.26	82.95	0.15	-11.93	6.69
SantaClara6085	0.11	-1.52	34.10	0.36	-8.78	12.05
SantaCruz6087	0.34	-1.92	31.55	0.50	-6.08	18.55
Shasta6089	0.17	-6.58	17.05	0.19	-9.16	11.28
Solano6095	0.13	-30.66	1.27	0.32	-13.98	4.40
Sonoma6097	0.14	1.76	50.38	0.33	-10.70	8.03
Stanislaus6099	0.32	-11.97	6.87	0.37	-10.85	7.46
Sutter6101	0.82	-26.64	1.53	0.20	-10.30	8.41
Tulare6107	0.33	-3.52	24.68	0.10	-9.94	9.18
Ventura6111	0.20	9.68*	85.50	0.29	-14.41	3.44
Yolo6113	0.46	14.01**	93.38	0.32	-10.29	8.60
Yuba6115	0.27	-17.20	3.82	0.26	-2.78	34.99

Table 7: County-Level Synthetic Control Average RMSFEs and Gap Estiamtes

	Popluation Weights		Housing Wealth Weights	
	RMSFE	Gap	RMSFE	Gap
Zillow REO Forc per 10,000 people	2.75	-91.89	2.22	-92.60
Zillow All Homes HP Growth	0.25	4.44	0.24	5.06
Zillow Top Tier HP Growth	0.20	2.60	0.19	2.24
Zillow Middle Tier HP Growth	0.23	3.08	0.22	3.12
Zillow Bottom Tier Returns	0.21	6.86	0.21	6.87
Auto Sales per 1000 people	0.30	-8.31	0.29	-7.88

Notes: Population and housing wealth weighted average RMSFEs and Gap Estimates across California counties.

Table 8: Zip Code Synthetic Control Average RMSFEs and Gap Estimates

				Pop Weights	
	N	RMSFE	Gap	RMSFE	Gap
Total	1196	0.68	7.55	0.53	9.52
Gap Percentile > 95	279	0.63	24.17	0.50	23.79
$90 >$ Gap Percentile ≤ 95	146	0.57	15.52	0.52	15.56
$85 >$ Gap Percentile ≤ 90	96	0.60	12.12	0.53	12.17

Notes: Unweighted and Population weighted RMSFEs and Gap Estimates across California zip codes. The estimation averages are shown for all zip codes and zip codes with a Gap Percentile greater than 95, greater than 90 and less than or equal to 95, and greater than 85 and less than or equal to 90. N is the number of California zip codes.

Table 9: Percentage of Zip Codes in each Region by Percentile

	Central	Inland	LosAngeles	Northern	SanDiego	SouthLA	Other
Gap Percentile > 95	16.85	45.10	41.82	11.38	0.00	41.84	14.40
$90 >$ Gap Percentile ≤ 95	10.67	11.76	18.64	8.75	0.00	17.35	13.60
$85 >$ Gap Percentile ≤ 90	7.30	6.86	13.18	5.91	0.00	12.24	6.40
Total Number of Zip Codes	178	102	220	457	16	98	125

Notes: For each region, the percentage of zip codes with a Gap Percentile greater than 95, greater than 90 and less than or equal to 95, and greater than 85 and less than or equal to 90. The bottom row shows the total number of zip codes in each region.

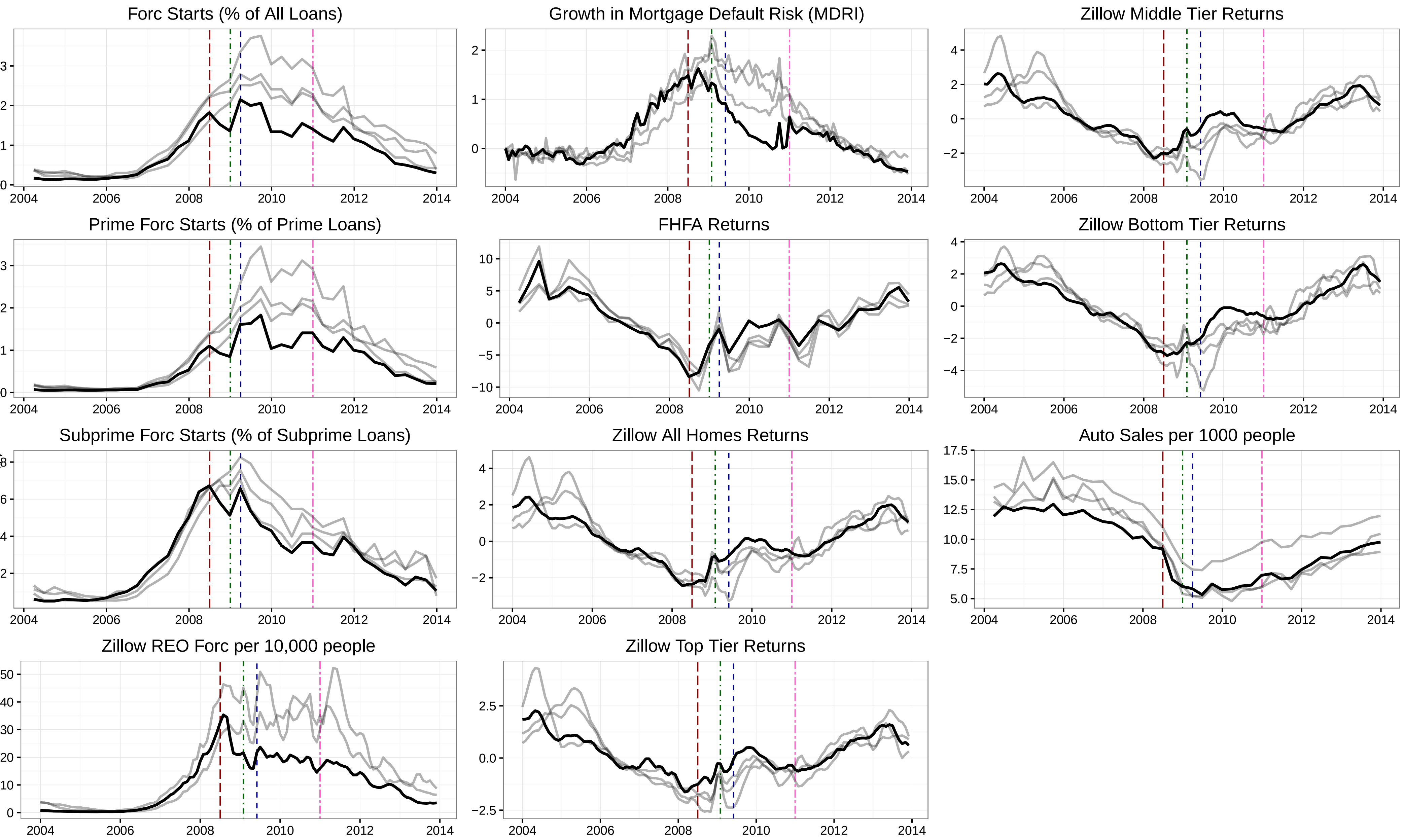
Table 10: Probability of Denial and Loan Volume Growth After the CFPLs

	Prob(Deny)				Loan Growth (\$)		Loan Growth (Num)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CA	-0.129^*	-0.116^*	-0.146^*	-0.148^*	0.110^*	0.049^*	0.186^*	0.193^*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.018)	(0.018)	(0.023)	(0.019)
$\Delta\log(\text{HP})_{t-1}$	0.120^*	0.245^*	0.149^*	0.300^*				
	(0.001)	(0.001)	(0.002)	(0.002)				
$\Delta\log(\text{HP})_{2008\_2009}$					0.098	0.206^*	-0.149^*	-0.157^*
					(0.048)	(0.043)	(0.053)	(0.040)
$\Delta\log(\text{HP})_{2010\_2011}$					0.376^*	0.690^*	0.675^*	0.362^*
					(0.098)	(0.076)	(0.118)	(0.073)
$\Delta\log(\text{HP})_{2012\_2014}$					0.042	0.246^*	0.198	0.075
					(0.076)	(0.059)	(0.089)	(0.051)
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	AZ,CA, FL,NV Loan Level	CA,CO, NY,TX Loan Level	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
Estimation Method	OLS	OLS	Probit	Probit	OLS	OLS	OLS	OLS
Observations	19,958,754	13,671,107	19,958,754	13,671,107	1,220	2,119	1,220	2,119
Adjusted R^2	0.350	0.276			0.873	0.802	0.688	0.651

Notes: Regressions of the probability of mortgage denial and zip code level loan volume growth on an indicator for California and controls. Columns 1 and 2 show regressions (linear probability model) of the probability of denial on an indicator for California and controls. Controls include the log of applicant income and the of the 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, applicant sex, the institution that purchased the loan after origination, the lien status of the loan, and owner occupancy. The samples include all loans in AZ, CA, FL, and NV (column 1) and CA, CO, NY, and TX (column 2) for all loan from 2009 to 2014. Columns 3 and 4 re-estimate the models in columns 1 and 2, but report marginal effects from a probit estimation (Partial effects at the average (PEA)). Columns 5 - 6 and 7 - 8 show regressions where dollar loan volume growth or the growth in the number of loans represents the dependent variable. 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; IRS income and population growth; Census zip code population density in 2010; and Zillow house price growth for 2008-2009, 2010-2011, and 2012-2014. Heteroskedasticity-robust standard errors are in parentheses. An asterisk represents a p-value less than 0.01.

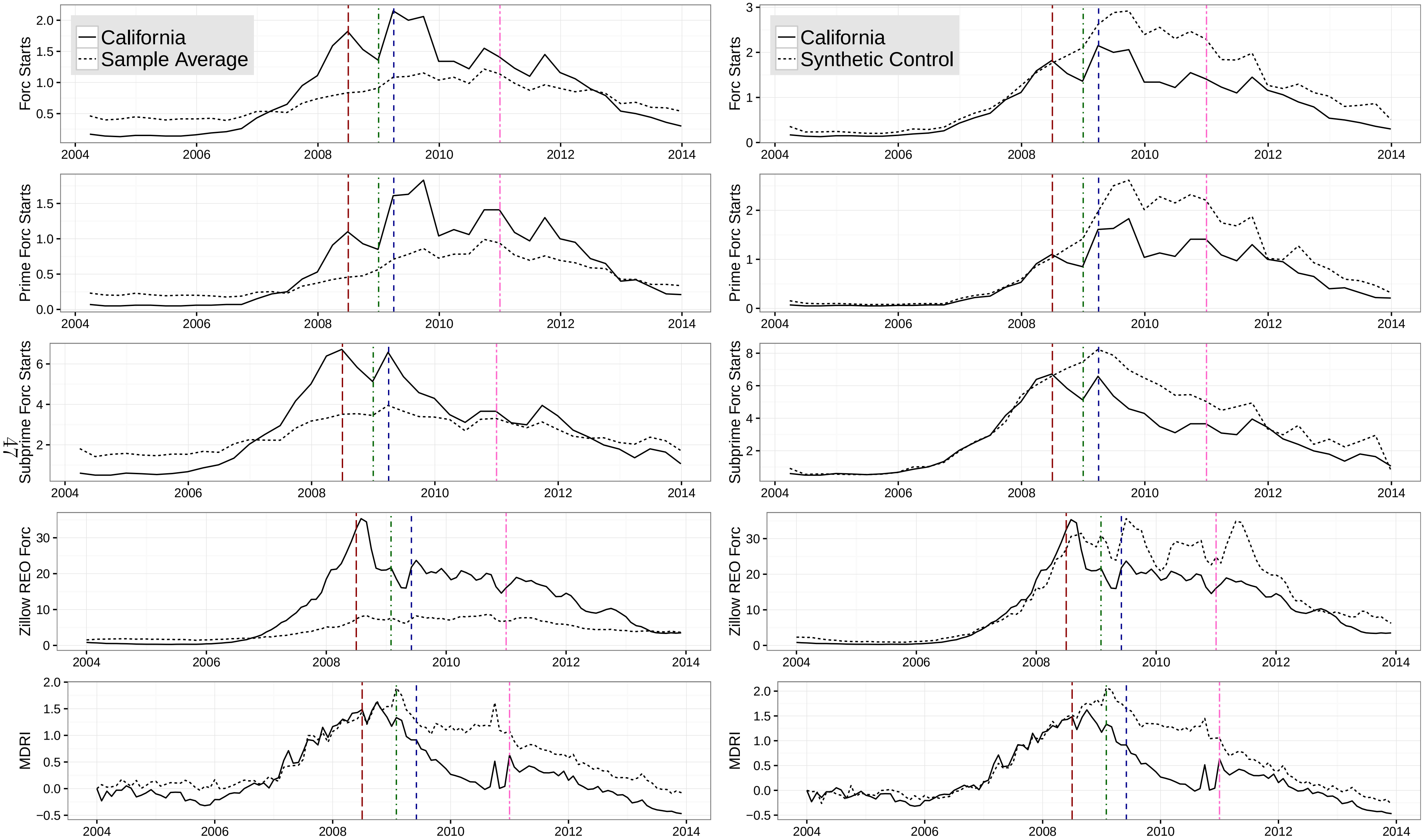
B Figures

Figure 1: Sand States Foreclosure Starts, the HDI, Housing Returns, and Auto Sales



Notes: Plots of foreclosure starts, housing distress, housing returns, and auto sales for Arizona, California, Florida, and Nevada. The bold black line is California, the gray lines represent Arizona, Florida, and Nevada. The long-dashed-red vertical line signifies the passage CA-1137 in 2008Q3 (2008M07); the dash-dot-green vertical line represents the CFPA policy announcement date in 2009Q1 (2009M02), the dashed-blue vertical line is the CFPA implementation date in 2009Q2 (2009M06), and the two-dashed-pink line represents the sunset date for the CFPA and the end of our policy period in 2010M12 (2010Q4).

Figure 2: Path Plots–Treated Unit, Sample Average, and Synthetic Control



Notes: **This figure extends over two pages**. See the notes for figure 1. The path plots of the Treated Unit (California), the Sample Average and the Synthetic Control. The left plot shows path of California (CA, solid line) versus the sample average (dotted line); the right panel shows the path of California (CA, solid line) versus its Synthetic Control (dotted line).

Figure 2 Continued

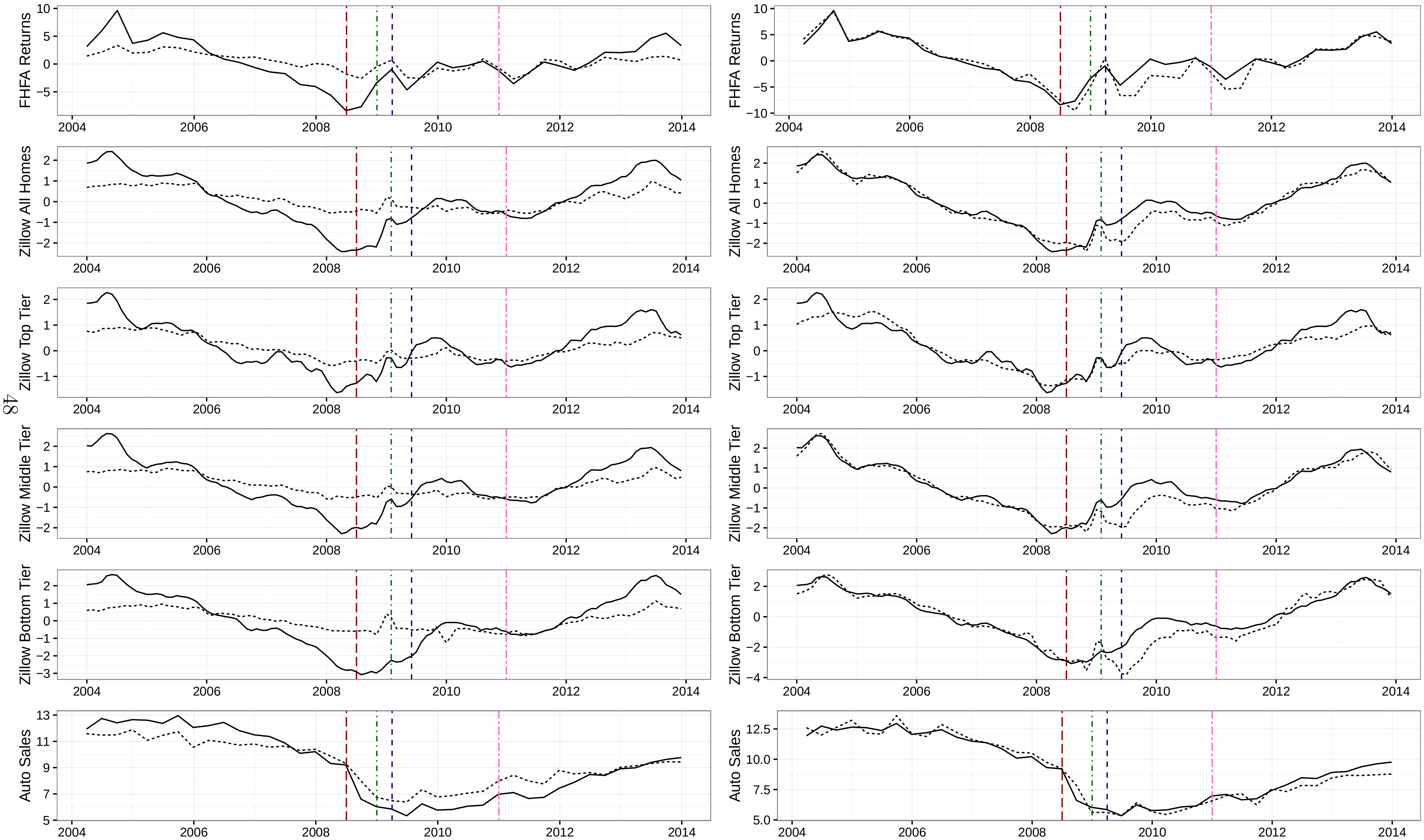
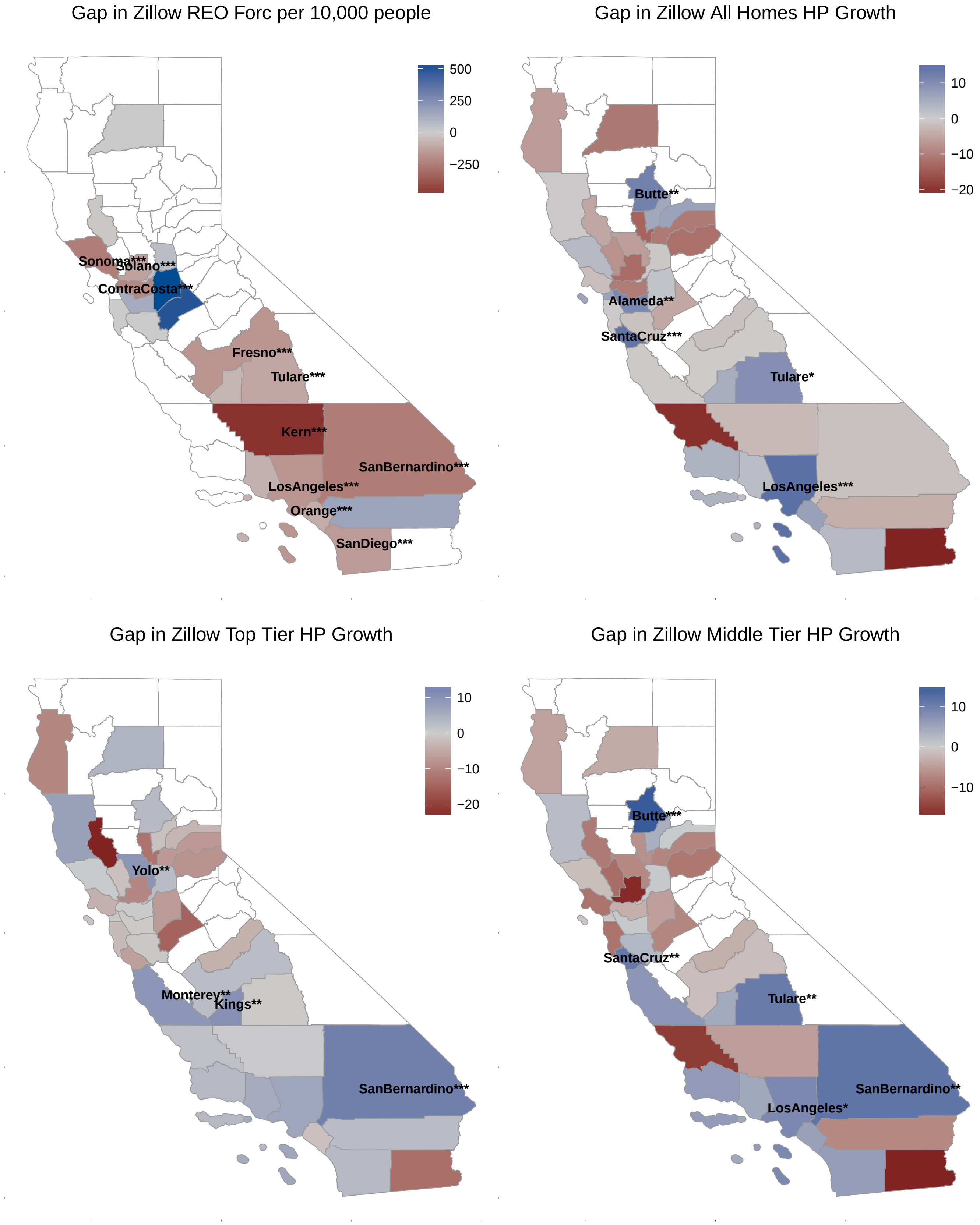


Figure 3: California County-Level Gap in Foreclosures, House Price Growth, and Auto Sales



Notes: **Continued on next page**. Choropleth plots of the Gap in California county-level foreclosures, house price growth, and auto sales during the CFPA period.

Figure 4: Figure 4 Continued

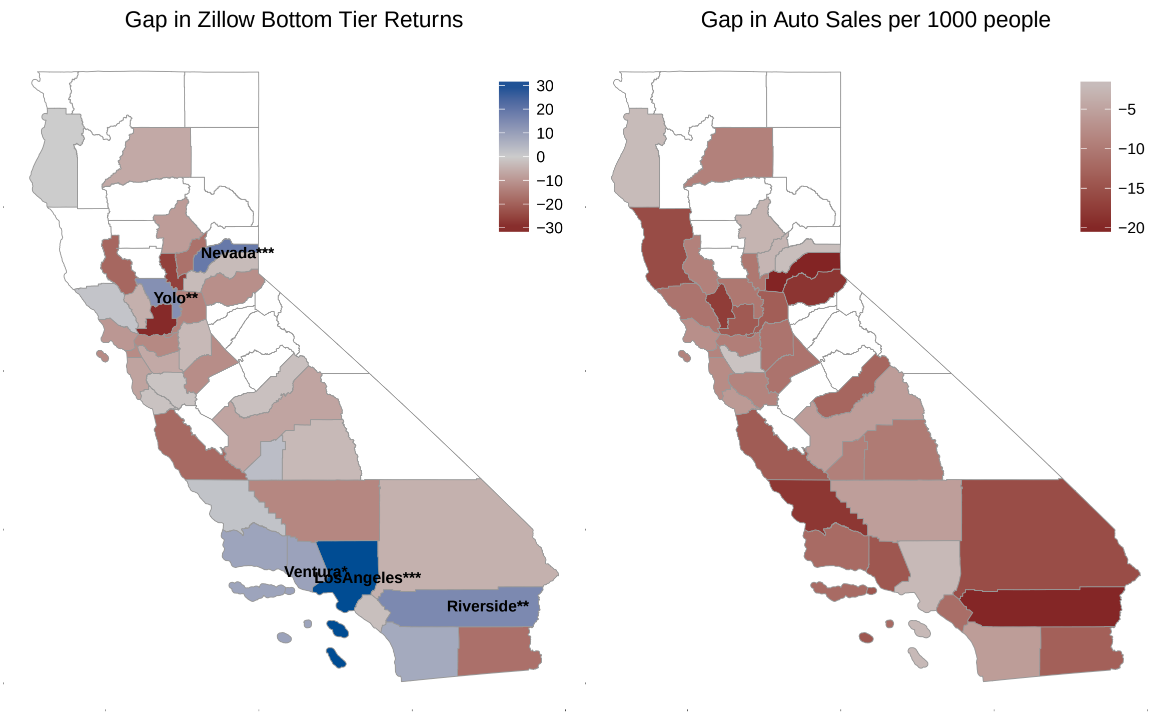
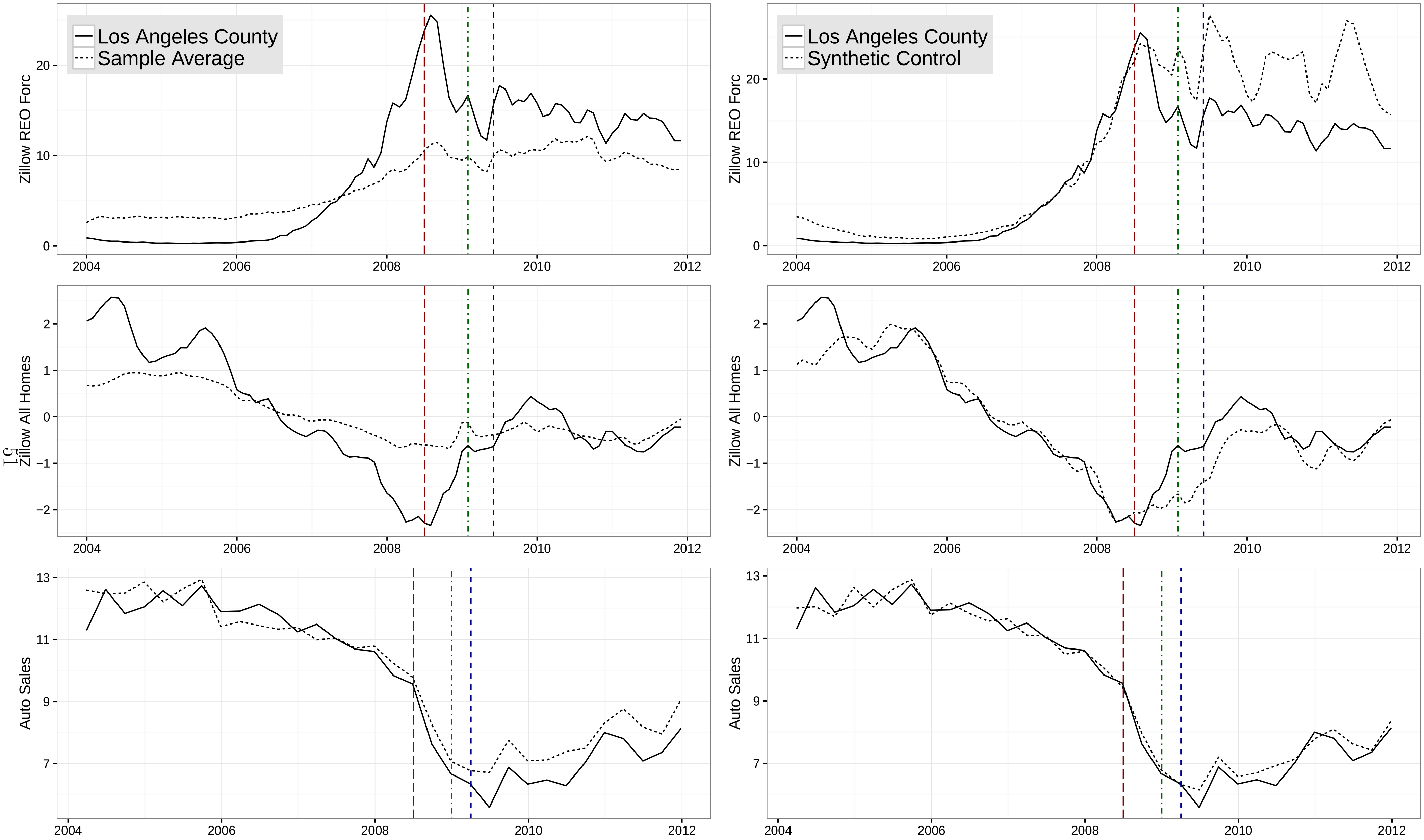
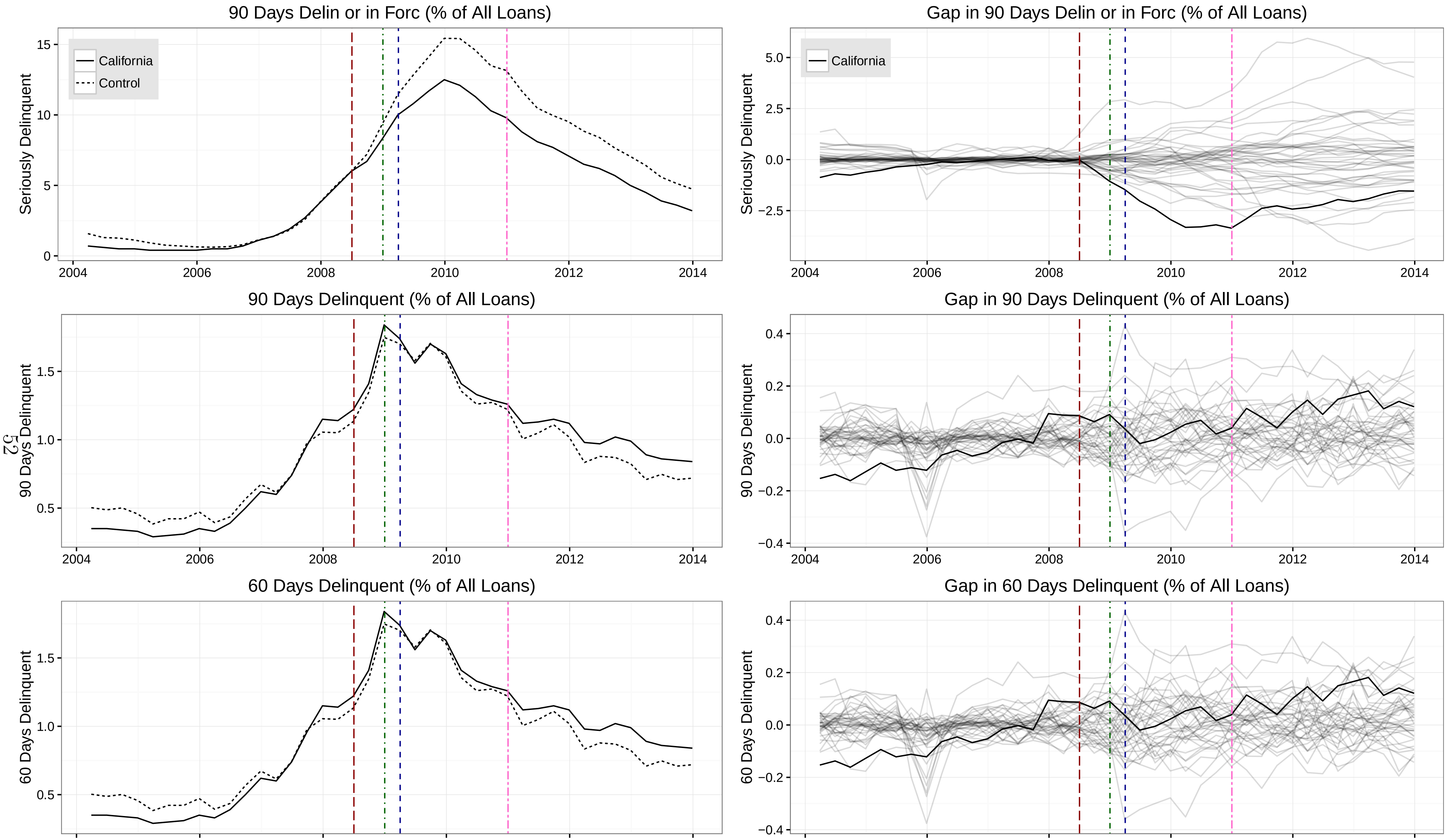


Figure 5: Path Plots – Los Angeles County, Sample Average, and Synthetic Control



Notes: Path plots for Los Angeles County for Zillow REO Foreclosures per 10,000 people, Zillow All Homes returns, and auto sales per 1000 people. The left plot shows path of Los Angeles (solid line) versus the sample average (dotted line); the right panel shows the path of Los Angeles County (solid line) versus its Synthetic Control (dotted line). The long-dashed-red vertical line signifies the passage CA-1137 in 2008Q3 (2008M07); the dash-dot-green vertical line represents the CFPA policy announcement date in 2009Q1 (2009M02), and the dashed-blue vertical line is the CFPA implementation date in 2009Q2 (2009M06).

Figure 6: Path Plots and Permutation Tess for California – Seriously Delinquent, 90 Days Delinquent, and 60 Days Delinquent



Notes: Path plots and permutation tests for California for the portion of mortgages that are Seriously Delinquent, 90 Days Delinquent, and 60 Days Delinquent. The left panel shows the path plots of California versus its Synthetic Control. In the right plot, California is the dark solid line; the gray lines are the permutation tests.

C Appendix: Data List

Table 11: Data List

Mnemonic	Description	Frequency	Source
	State-Level Data		
	Foreclosure Starts–Percentage of loans that enter foreclosure	Q	MBA
	Prime Forelcosure Starts	Q	MBA
	Subprime Foreclosure Starts	Q	MBA
	Housing Distress Index (HDI)	M	CGL
	FHFA House Returns	Q	FHFA
	Zillow Returns (All Homes, Top Tier, Middle Tier, Bottom Tier, Condos)	M	Zillow
	Auto Sales Per 1000 People	Q	Polk
**BP1FHSA	Housing Starts	Q	FRED
**NA	Nonfarm Payrolls	Q	FRED
**OTOT	Total Personal Income	Q	FRED
	Saiz Elasticity	NA	Saiz
**POP	Population Density in 2005	NA	FRED,Census
	County-Level Data		
	Zillow Returns (All Homes, Top Tier, Middle Tier, Bottom Tier, Condos)	M	Zillow
	Auto Sales Per 1000 People	Q	Polk
	Median Household Income	Y	ACS
	Unemployment Rate	Y	BLS
	Population Density in 2005	NA	ACS,Census
	Zip Code Level Data		
	Zillow Returns (All Homes)	M	Zillow
	Income per household	NA	IRS
	Population Density in 2007	NA	IRS,Census

Notes: MBA is the Mortgage Bankers' Association; CGL is Chauvet, Gabriel, and Lutz (2015); FRED is the FRED Database of the Federal Reserve Bank of St. Louis; Census is the US Census; ACS is the American Community Survey; BLS is the Bureau of Labor Statistics; IRS is the IRS Statistics of Income.

Table 12: California Counties and FIPs codes

Alameda - 6001	Sacramento - 6067
Butte - 6007	San Bernardino - 6071
Contra Costa - 6013	San Diego - 6073
El Dorado - 6017	San Francisco - 6075
Fresno - 6019	San Joaquin - 6077
Humboldt - 6023	San Luis Obispo - 6079
Imperial - 6025	San Mateo - 6081
Kern - 6029	Santa Barbara - 6083
Kings - 6031	Santa Clara - 6085
Lake - 6033	Santa Cruz - 6087
Los Angeles - 6037	Shasta - 6089
Madera - 6039	Solano - 6095
Marin - 6041	Sonoma - 6097
Mendocino - 6045	Stanislaus - 6099
Monterey - 6053	Sutter - 6101
Napa - 6055	Tulare - 6107
Nevada - 6057	Ventura - 6111
Orange - 6059	Yolo - 6113
Placer - 6061	Yuba - 6115
Riverside - 6065	