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Abstract

We study how regulatory oversight by the Consumer Financial Protection Bureau (CFPB) affects mortgage credit supply and other aspects of bank behavior. We use a difference-in-differences approach exploiting changes in regulatory intensity and a size cutoff below which banks are exempt from CFPB scrutiny. CFPB oversight leads to a reduction in lending in the Federal Housing Administration (FHA) market, which primarily serves riskier borrowers. However, it is also associated with a lower transition probability from moderate to serious delinquency, suggesting that tighter regulatory oversight may reduce foreclosures. Our results underscore the trade-off between protecting borrowers and maintaining access to credit.

JEL Classification: G21, G28, D18

Keywords: Consumer financial protection, Regulation, Mortgages, Servicing, credit supply

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1 Introduction

The Great Recession and the associated mortgage default crisis catalyzed policymaker interest in consumer financial protection. The centerpiece of the U.S. policy response has been the creation of a new regulatory agency, the Consumer Financial Protection Bureau (CFPB), dedicated to overseeing and enforcing consumer financial protection laws. Since its debut in 2011, the CFPB has actively exercised its powers, for example conducting more than 200 public enforcement actions and recovering more than $12bn in relief payments for consumers.

In this paper, we evaluate the effects of the CFPB's examination, supervision and enforcement powers (or more succinctly, "CFPB oversight") on bank credit supply and other aspects of bank behavior. We focus on the mortgage market, which is the largest consumer credit market and the subject of almost a third of CFPB enforcement actions (Peterson, 2019). Our identification strategy makes use of the fact that banks with less than $10bn in total assets are exempt from CFPB oversight. Using a difference-in-differences approach, we examine changes in lending and servicing outcomes for banks above and below this $10bn size threshold around two events: (i) the formation of the CFPB in July 2011, and (ii) the November 2016 federal election, which led to a relaxation in the intensity of CFPB oversight and a sharp drop in the number of enforcement actions.

We find that CFPB oversight has little effect on total mortgage lending but causes economically significant changes in the *composition* of lending. In particular, affected banks withdraw from Federal Housing Administration (FHA) lending, a market where mortgage borrowers are typically lower-income and often first-time homebuyers, and where lending involves elevated legal and regulatory risk. Quantitatively, the formation of the CFPB reduced the FHA market share of covered banks by 4.5-5.5 percentage points, equivalent to 10-15% of the pre-period market share of these banks within our sample. This decline is offset by an increase in "jumbo" lending, high-balance mortgages to borrowers who are generally affluent and have strong credit histories. Subsequently, this decline in FHA lending is partially reversed in the quarters after the 2016 federal election.

Beyond these effects on credit supply, we also find evidence suggesting that CFPB oversight leads to an improvement in mortgage servicing practices. Specifically, FHA mortgages from CFPB-supervised banks become 16% more likely to avoid progressing from moderate to serious delinquency (from 60 days to 90+ days delinquent) conditional on loan and borrower characteristics. This is consistent with a shift to more diligent servicing, such as being more proactive in contacting and supporting delinquent borrowers or referring them to credit counselling. Notably, we find no corresponding difference in outcomes for *initial* transitions into delinquency (where servicing quality is unlikely to play a role), suggesting our results are indeed about servicing rather than unobserved variation in borrower credit quality.

CFPB oversight also leads to shifts in lending *within* banking organizations. We find evidence that the creation of the CFPB in mid-2011 led to a form of regulatory arbitrage in which small banking firms shift FHA lending from nonbank subsidiaries (which became subject to CFPB scrutiny) to bank subsidiaries (which were exempt). This result highlights the potentially unintended consequences of applying different rules across affiliates within a financial conglomerate (see Demyanyk and Loutskina, 2016 and Acharya, Schnabl, and Suarez, 2013 for related evidence).

Our results speak to an active policy debate about the costs and benefits of consumer financial protection and more specifically the CFPB.[1] Critics argue that the CFPB's activities increase costs and legal risk, thereby reducing the supply of credit to consumers (U.S. Chamber of Commerce, 2018; Neugebauer and Williams, 2015). Supporters argue that CFPB oversight has been effective in deterring and punishing deceptive and abusive practices (Cordray, 2017). Our estimates provide some support for both these perspectives—we find evidence suggesting that CFPB oversight leads to an improvement in servicing practices which may reduce inefficient foreclosures, but also that it induces a contraction in lending to risky borrowers during a period where some have argued mortgage lending is too "tight".[2]

Our analysis relies primarily on data collected under the Home Mortgage Disclosure Act (HMDA), and focuses on banks with \$1bn-25bn in total assets, a window around the \$10bn size threshold for CFPB oversight. We estimate the probability that a mortgage is originated by a CFPB-supervised bank, controlling for different combinations of loan, borrower and geographic controls, and then trace out how this conditional probability

[1] We note that aside from examination, supervision and enforcement powers, the CFPB also has rule making authority, which it has exercised extensively (introducing for instance the TILA-RESPA integrated disclosure rule, also known as TRID, or the qualified mortgage requirements). Our results do not speak to the effects of these rules, since they apply to all lenders.

[2] For example, the Urban Institute's Housing Credit Availability Index (HCAI) suggests mortgage credit conditions were restrictive during the period of our study (see https://www.urban.org/policy-centers/housing-finance-policy-center/projects/housing-credit-availability-index).

evolves around each event date (July 2011 in the case of the formation of the CFPB, and November 2016 in the case of the federal election of that year). We find no evidence of pre-trends in FHA lending prior to each event date, but lending shifts significantly in the quarters after the event, persisting until the end of the relevant sample period.

We then conduct several tests to evaluate alternative explanations for our findings. First, we examine whether our results are due to other regulations applying above a $10bn size threshold, specifically the requirement to form a risk committee, conduct internal stress tests, and comply with a cap on debit interchange fees. Since these other rules are not focused on consumer lending, we implement a placebo test in which we re-estimate our regression model for small business loans, which are not part of the CFPB's remit. Our results do *not* generalize to small business lending, implying our findings are driven by mortgage-specific factors rather than a general increase in regulatory burden. Second, we investigate whether our results are affected by banks' incentive to bunch below the $10bn size cutoff. FHA mortgages are securitized and do not add to bank size, thus we would not anticipate bunching to account for our results; consistent with this prior, our results are similar if we exclude banks near the $10bn threshold.[3] Third, our results are robust to adjusting the size window for defining the bank sample, choices about weighting, or restricting the sample to purchase mortgages.

This paper contributes to a growing literature on consumer financial protection. Campbell et al. (2011) argue that consumer financial protection regulation can improve welfare because of information asymmetries, behavioral biases, and externalities such as foreclosure spillovers. Our evidence suggests that CFPB oversight may indeed improve servicing practices, which in turn could reduce inefficient mortgage foreclosures. Our analysis thereby complements studies of individual laws and regulations designed to protect consumers, such as research on the CARD Act by Agarwal et al. (2015) and Debbaut, Ghent, and Kudlyak (2016).[4]

Our results are also related to research studying the causal effects of bank supervi-

[3] Although not our main focus, we also show that while the bank size distribution exhibits bunching below $10bn after the passage of Dodd-Frank, this bunching itself largely disappears after the 2016 federal election, additional evidence that banks perceived a relaxation of the regulatory environment post-election.

[4] Other examples include studies of anti-predatory-lending laws, payday lending restrictions, and regulations that cap household leverage (e.g. Ho and Pennington-Cross, 2006; Melzer, 2011; Morgan, Strain, and Seblani, 2012; Di Maggio and Kermani, 2017; DeFusco, Johnson, and Mondragon, 2020). Unlike these studies and the earlier-cited research on the CARD Act, our focus is on the overall effects of CFPB examination, supervision and enforcement activities, rather than the effects of any one individual law or regulation.

sion (e.g. Agarwal et al., 2014; Eisenbach, Lucca, and Townsend, 2020; Hirtle, Kovner, and Plosser, 2020; Ivanov and Wang, 2020; Kandrac and Schlusche, 2020). This literature focuses on prudential supervision and its effects on risk and financial stability, however, whereas we focus on supervision related to consumer financial protection.

Several other papers examine bank outcomes around regulatory size thresholds introduced in the Dodd-Frank Act, in particular the \$50bn threshold (later raised to \$100bn) applying to enhanced supervision and the Comprehensive Capital Analysis and Review (Acharya, Berger, and Roman, 2018; Bouwman, Hu, and Johnson, 2018). Like us, Bouwman et al. (2018) and Kay, Manuszak, and Vojtech (2018) also study bank outcomes around the \$10bn asset size threshold, although they focus on other outcomes such as asset growth, profitability and debit interchange fee income, and do not use microdata to study mortgage lending or servicing as we do here.

We are not aware of other quantitative studies of the impact of CFPB supervision and enforcement activities, but research by legal scholars does evaluate this question qualitatively (e.g., Levitin, 2013). Within the economics literature, DeFusco et al. (2020) study the effects of the "ability-to-repay/qualified mortgage" rule introduced by the CFPB in 2014, which applies to all lenders regardless of size. Other research studies the CFPB's public complaints database. Begley and Purnanandam (2020) use the database to study the relation between the frequency of mortgage-related consumer complaints and local population characteristics. Dou and Roh (2020) study the effect of the public release of the complaints database in 2013 on mortgage applications.

Finally, our results provide additional evidence that legal and regulatory risk affects mortgage credit supply, consistent with Buchak et al. (2018), D'Acunto and Rossi (2020), Fuster, Lo, and Willen (2017), Gissler, Oldfather, and Ruffino (2016) and Hartman-Glaser, Stanton, and Wallace (2014). This prior research focuses on factors such as regulatory uncertainty and investor lawsuits or "put-backs", however, rather than risks associated with CFPB oversight. These and other papers also document general changes in mortgage lending since the Great Recession, such as the striking growth in nonbank lending at the expense of large banks (e.g., Begley and Srinivasan, 2020; Gete and Reher, 2018, 2020; Kim et al., 2018).

2 Background on the CFPB

The creation of the CFPB was a cornerstone of the Dodd-Frank Act signed into law in July 2010. Federal responsibility for consumer financial protection had previously been shared across a range of agencies including the Federal Reserve, Office of the Comptroller of the Currency, Department of Housing and Urban Development, Veterans Administration and Federal Trade Commission. Warren (2007) and Levitin (2013) argue that this fragmented system led to weak overall regulation because consumer financial protection was not the primary mission of any federal agency, and because it created opportunities for regulatory arbitrage and charter shopping. In some cases, consumer protection may even conflict with other regulatory goals, such as bank safety and soundness.

The CFPB has authority over both banks and nonbanks, with powers in three areas (see 12 U.S. Code 5515):

1. Rule making. The CFPB makes rules under the federal consumer protection laws such as the Truth in Lending Act and the Home Ownership and Equity Protection Act. It also has the organic authority to define "unfair, deceptive or abusive acts or practices" (UDAAPs), which are prohibited under the Dodd-Frank Act.

2. Supervision and examination. The CFPB has the power to solicit information and send examiners to study the records of financial firms, interview employees, and so on.

3. Enforcement. The CFPB can pursue enforcement actions against firms in breach of consumer protection laws, leading to the recovery of civil money penalties, refunds, injunctive relief or other remedies. The CFPB cannot undertake enforcement actions alone, however; it must act jointly with other agencies.

2.1 CFPB supervision and enforcement activities

The CFPB began operating on July 21, 2011, and quickly began exercising its supervisory and enforcement powers. From 2012-2019 the agency undertook 217 public enforcement actions leading to more than $12bn in relief payments to consumers as well as a range of other penalties (Peterson, 2019).

While supporters lauded the CFPB's active approach, critics argued that the Bureau's oversight activities were intrusive and adversarial, imposing heavy costs on financial firms. For instance, the U.S. Chamber of Commerce (2013) criticized the presence of enforcement attorneys on supervisory exams, and argued that the CFPB examination process was *"confusing, unnecessarily duplicative, inconsistent, and open-ended."* Another complaint was that the CFPB engaged in "regulation by enforcement", using enforcement actions against individual firms to define inappropriate practice rather than laying out clear industry-wide regulations ex ante (Kaplinsky, 2012; Mulvaney, 2018a; U.S. Chamber of Commerce, 2013).[5]

According to critics, the end result has been a contraction in the supply of financial services to consumers. For example, an article by two U.S. Representatives argued that the Bureau's actions *"harm consumer choice, decrease credit availability, and increase costs"* (Neugebauer and Williams, 2015). Defenders of the CFPB argued instead that the Bureau's oversight activities improved consumer welfare by discouraging deceptive and predatory behavior and by providing compensation to victims of financial malpractice.[6]

2.1.1 Post-2016 changes

The CFPB's approach to supervision and enforcement changed significantly following the 2016 federal election, which resulted in Republican majorities in the House and Senate and the election of a Republican President. Republican lawmakers had previously criticized the CFPB and had drafted legislation to raise the asset size threshold below which banks are exempt from CFPB oversight (e.g., via the Financial Regulatory Improvement Act of 2015). Shortly after the 2016 election, the incoming Administration reiterated its intention to roll back parts of the Dodd-Frank Act, furthering expectations that the powers of the CFPB would be reduced (e.g., Nussbaum, 2016; Gordon, 2016).

In 2017, CFPB Director Richard Cordray was replaced by Acting Director Mick Mul-

[5] For example, Kaplinsky (2012) writes that when the CFPB settled its first enforcement action, against the credit card lender Capital One, it simultaneously released a bulletin setting out appropriate practices for marketing of credit-card products, the same products which had been the subject of the enforcement action. Kaplinsky argues the Capital One enforcement action was being used as a demonstration in order to set industry-wide standards, bypassing the usual rulemaking process.

[6] Consistent with the view that the CFPB's oversight was changing behavior, a number of law and consulting firms posted public recommendations during this period that financial firms should modify their activities to mitigate the risks associated with CFPB supervision and enforcement, For instance Skadden, Arps, Slate, Meagher and Flom (2016) advises that *"In light of the CFPB's recent enforcement activity and anticipated rulemaking restricting arbitration agreements, consumer financial services companies would be well-advised to review consumer complaints as well as their policies and procedures to proactively address practices that may present enhanced risk of enforcement or consumer litigation."*

vaney and subsequently in 2018 by a new permanent Director, Kathy Kraninger. Associated with these leadership changes, CFPB public enforcement actions declined by 70%, and restitution payments to consumers declined by 85%.[7] For mortgages, the decrease in enforcement actions was even more striking—a decline in restitution payments of more than 99% after the departure of Director Cordray (Peterson, 2019). Reversing a consistent upward trend in employment at the Bureau, the CFPB also imposed a hiring freeze, and reduced its staffing by 15% between 2017:Q2 and 2019:Q1 (Hayashi, 2019).

Public statements also emphasized a change in enforcement strategy at the CFPB. For example, in written Congressional testimony, Acting Director Mulvaney wrote that *"The Bureau's new strategic priorities are to recognize free markets and consumer choice and to take a prudent, consistent, and humble approach to enforcing the law"* and that *"In another change, the Bureau practice of 'regulation by enforcement' has ceased"* (Mulvaney, 2018b). In a Wall Street Journal editorial, Mulvaney wrote that *"The days of aggressively 'pushing the envelope' are over"* (Mulvaney, 2018a).

3 Empirical strategy

Our identification strategy exploits the fact that the CFPB's supervision and enforcement powers do not extend to banks with less than $10bn in assets. Responsibility for overseeing consumer financial protection for these smaller banks remains with their prudential regulator, as was the case prior to Dodd-Frank. This carve-out from CFPB oversight only applies to depository institutions (i.e., commercial banks, savings banks, and credit unions). Nonbanks are subject to CFPB oversight regardless of their size.[8]

As illustrated in Figure 1, we use a difference-in-differences approach to examine lending behavior before and after the CFPB launches operations in July 2011, comparing a "treated" set of banks that become subject to CFPB oversight to a set of "control" firms below the $10bn size threshold. We use a similar strategy to study the effects of the changes

[7]Enforcement actions per week dropped from 0.72 during Cordray's term to 0.20 during Mulvaney's term, and to 0.38 in the early part of Kraninger's term. Consumer relief payments per week declined from $43.0m (Cordray) to $6.4m (Mulvaney) and $0.9m (Kraninger). Source: Peterson (2019).

[8]See sections 1025 and 1026 of the Dodd-Frank Act. The CFPB may however require the prudential regulator to provide information from its supervisory activities, and may at its discretion include examiners on a sampling basis as part of the examinations performed by the prudential supervisor. The CFPB is also expected to notify the prudential regulator if it believes a material violation of consumer financial law has occurred (see 12 U.S. Code 5516).

in CFPB supervision and enforcement strategy following the 2016 federal election. These changes relax the intensity of CFPB oversight for banks above the $10bn threshold, but do not affect smaller banks which were already exempt from CFPB scrutiny.

Figure 1: Empirical strategy

Bank Size	Regulator responsible for consumer financial supervision and enforcement:	
>$10bn	Prudential regulator	**CFPB**
<$10bn	Prudential regulator	Prudential regulator
	Pre-CFPB (up to Q2:2011)	Post-CFPB (Q3:2011 onwards)

We use quarterly lists of covered depository institutions posted on the CFPB website to determine which banks are subject to CFPB oversight. These lists reflect some nuances in the way the $10bn threshold is defined, in particular: (i) the asset size test applies to each bank individually, rather than the consolidated assets of the bank holding company (BHC); (ii) banks affiliated with a >$10bn bank are subject to CFPB oversight even if they themselves are smaller than $10bn;[9] and (iii) a bank crossing through (dropping below) the $10bn threshold must remain above (below) the threshold for four consecutive quarters before becoming subject to (exempt from) CFPB oversight.[10]

3.1 Sample construction

Our bank sample is the set of commercial banks and savings banks with assets between $1bn and $25bn as of the end of the quarter prior to the event date. The CFPB launched operations on July 21, 2011, hence the preceding quarter is 2011:Q2; the federal election took place on November 8, 2016, hence the preceding quarter is 2016:Q3. We focus on

[9] E.g. if a BHC owns two bank subsidiaries, with $16bn and $8bn in assets respectively, both would be subject to CFPB oversight. But if a BHC with assets of $16bn is comprised of two banks each with $8bn in assets, neither bank would be subject to CFPB oversight.

[10] As a cross check, we also manually calculate which banks would be expected to be subject to CFPB oversight based on the rules set out in the Dodd-Frank Act, using bank asset size from Call reports and National Information Center data on BHC structure. Our calculations closely correspond to the the lists on the CFPB website although the two do not match exactly. We rely on the CFPB lists under the assumption that they most closely reflect actual supervisory practice.

banks in the neighborhood of the $10bn threshold to obtain a more homogeneous sample and to avoid confounding effects from additional regulations which affected larger banks. To this end we also exclude any bank which is a subsidiary of a large bank holding company (BHCs) with assets exceeding $50bn.[11]

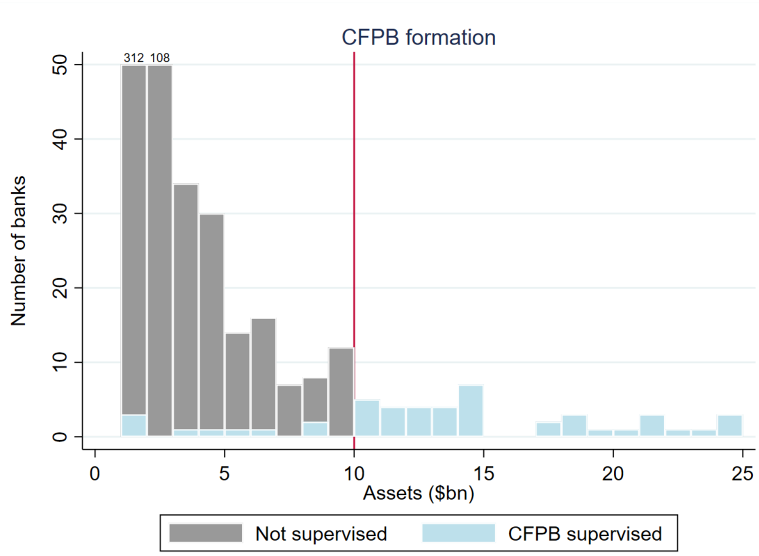
We classify a bank as being subject to CFPB oversight if it is a covered firm on the event date, and then hold this classification fixed. We use this static approach so that the treatment status of a bank is not affected by any endogenous size response of the bank to changes in regulatory intensity. In practice, our results are similar if we allow the CFPB oversight indicator to vary dynamically, because most banks' status does not change during the two relatively short time windows we study. Bank assets and other financial information are drawn from the Call Report and Thrift Financial Report. Information on bank structure and the identity of the BHC parent are taken from the National Information Center database. Consolidated BHC assets are obtained from the FR Y-9C.

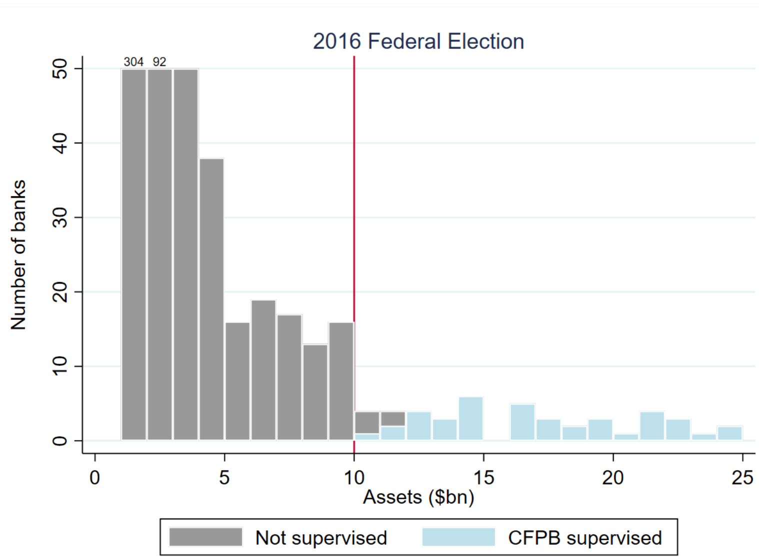
Figure 2 presents histograms by size of our bank sample for each event. For the sample defined as of June 30, 2011, there are 48 banks which become subject to CFPB oversight when the CFPB launched operations in July. Of these, 39 have assets between $10bn and $25bn, and nine are smaller than $10bn but have an affiliate larger than $10bn. The other 532 banks are smaller organizations exempt from CFPB oversight. In the second sample, used to analyse the post-2016-election relaxation of supervisory intensity, there are 40 banks subject to CFPB oversight, and 581 exempt banks. Note that the latter sample contains a small number of non-CFPB-supervised banks exceeding the $10bn threshold, because these banks had not yet crossed the threshold for more than four consecutive quarters. Summary statistics and more details about how the bank sample is constructed are provided in Internet Appendix A.

There are many more CFPB-exempt banks in both samples, reflecting the mass of small community banks in the U.S. However, the above-vs-below $10bn samples are more balanced measured in terms of the dollars of total mortgage lending, as we show below. Also noticeable in the figures is the bunching of banks just below the $10bn asset size threshold in the 2016:Q3 sample. This bunching, which is not evident in 2011:Q2, reflects an unwillingness of banks to bear the additional regulatory costs of crossing the $10bn size threshold (Morgan and Yang, 2016; Bouwman et al., 2018; Alvero, Ando, and Xiao, 2020).

[11] Following Dodd-Frank, BHCs larger than $50bn became subject to enhanced supervision as well as supervisory stress testing under the Comprehensive Capital Analysis and Review (CCAR). Large BHCs are likely different in other dimensions as well, related to their funding sources and economies of scale/scope.

Figure 2: Size Distribution of Banks Around $10bn Threshold





Note: Top panel shows asset size distribution of our bank sample as of June 30, 2011; bottom panel shows distribution of our second bank sample as of September 30, 2016. Numbers above first two bars indicate number of banks in size groups $1-2bn and $2-3bn, respectively.

We present further analysis on this point in Section 7.

3.2 HMDA data

We merge our two bank-level samples with loan-level data on mortgage applications and originations collected under the Home Mortgage Disclosure Act (HMDA). HMDA data are widely used for mortgage research, and include information on nearly all mortgage originations in the United States.[12] Application information includes the lender identity, property location, loan amount, and borrower characteristics such as income, race and gender. The data also report whether the application resulted in an origination and if so, whether the loan was sold/securitized in the same calendar year. The restricted-use version of the data available to users within the Federal Reserve System also contains exact application and "action" (e.g. origination) dates, meaning we can track the evolution of lending more precisely than in the public HMDA data, which only contains calendar year indicators. See Avery, Brevoort, and Canner (2007) for a detailed description of HMDA data and its strengths and weaknesses for use in research.

We use a concordance developed by Robert Avery to identify HMDA mortgage originations by each bank (see Internet Appendix A for more details). This match identifies bank lending as well as loans by the bank's nonbank affiliates. We exclude nonbank affiliate loans from our main analysis, because these affiliates are subject to CFPB oversight even if their bank parent is not. In Section 6, however, we explicitly study substitution between nonbank and bank affiliates induced by the exemption of small banks from CFPB scrutiny.

4 Effects on mortgage lending

In this section we study the effect of CFPB oversight on the volume and composition of bank mortgage lending. The mortgage market is a natural laboratory to study these effects, because mortgages are the largest component of household debt, and are the subject of almost a third of CFPB enforcement actions as well as a large share of consumer complaints about financial institutions (Begley and Purnanandam, 2020).

In addition to studying total lending, we are particularly interested in the FHA market,

[12]HMDA reporting is mandatory for banks with assets above a low asset size cutoff (e.g. $40 million in 2011) that have a branch in a metropolitan statistical area and made at least one mortgage loan in a given year. See https://www.ffiec.gov/hmda/reporter.htm for detailed reporting criteria.

which is the main source of subprime mortgage credit during our study period (Adelino, McCartney, and Schoar, 2020). FHA borrowers have lower incomes and higher default risk, and are often first-time home buyers. As a result, FHA lending exposes banks to elevated legal and regulatory risk. These risks were particularly salient during our study period because FHA lenders were subject to large legal settlements in the wake of the 2008 crisis, totalling at least $7bn in damages.[13] Many banks retreated from the FHA market after the crisis, sometimes directly citing legal and regulatory risk as the reason why (Dimon, 2017; Whalen, 2018). Today, FHA lending is dominated by nonbanks.

4.1 Mortgage lending around the start of CFPB oversight

Figure 3 plots the dollar volume of mortgage lending by the CFPB-supervised and non-supervised banks in our sample in the period around the formation of the CFPB in July 2011. Loan volume is normalized to average 100 in the pre-CFPB period.

There is no obvious evidence of a drop in total lending by CFPB-supervised banks relative to the control group after the CFPB starts operating (panel A). Lending by both groups fluctuates significantly over time, but co-moves closely. If anything, lending is slightly higher for the supervised banks in the post-2011:Q2 period.

In contrast, panel B shows a significant drop in FHA lending among banks that become subject to CFPB oversight, compared to the control group. Although the two series do not co-move as closely, they exhibit the same basic pattern of a rise in 2010 followed by a decline in the first half of 2011. After the CFPB begins operations, FHA lending by CFPB-supervised banks is consistently lower than for the comparison group.

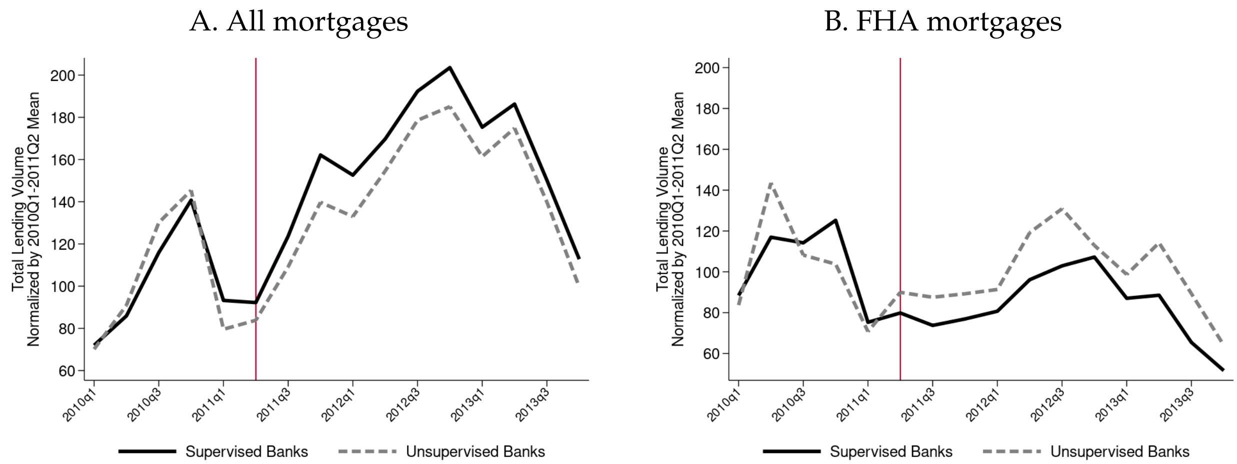
4.1.1 Regression analysis

Loan-level regression analysis allows us to more precisely investigate these differences and control for geographic variation in demand. Using the same sample filters as Figure 3, we estimate loan-level linear probability models of the form:

$$CFPBsupervised_{ict} = \alpha_c + \beta \cdot post2011Q2_t + \Gamma X_{ict} + \varepsilon_{ict}, \tag{1}$$

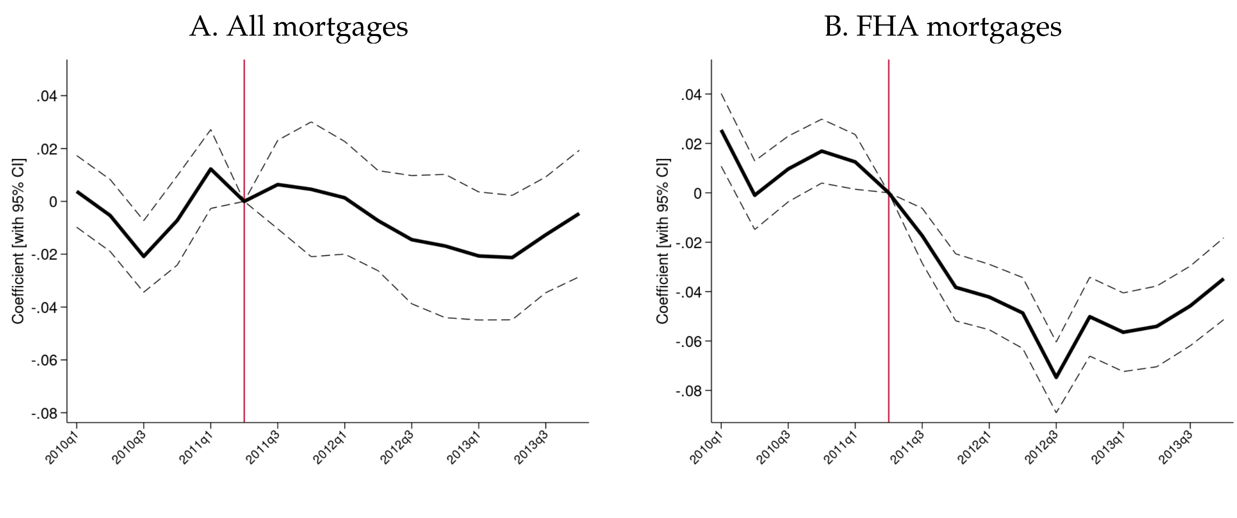
[13]This $7bn figure represents FHA legal settlements against banks under the False Claims Act. Source: FHA Director Brian Montgomery, quoted in https://www.housingwire.com/articles/hud-doj-changing-use-of-false-claims-act-in-order-to-bring-big-banks-back-to-fha-lending/.

Figure 3: Total mortgage originations of CFPB-supervised and CFPB-unsupervised banks around the introduction of the CFPB.



Note: Sample includes banks with assets between \$1bn and \$25bn as of 2011:Q2. Total originations of each group are normalized by the average volume over 2010:Q1-2011:Q2.

Figure 4: Evolution of probability that a mortgage was originated by a CFPB-supervised bank around the formation of the CFPB in July 2011.



Note: Quarterly series show estimated quarterly coefficient (relative to 2011:Q2) and 95% confidence intervals. Regressions also control for census tract fixed effects and loan-level controls, and observations are weighted by loan amount.

where $CFPBsupervised_{ict}$ is a dummy equal to 1 if mortgage i in census tract c originated in quarter t was originated by a bank that became subject to CFPB oversight in July 2011. $post2011Q2_t$ is a dummy variable equal to 1 during the time period when the CFPB is active (2011:Q3 onwards); α_c is a set of census tract fixed effects; and X_{ict} is a set of loan characteristics included in some specifications. Our coefficient of interest is β; a negative estimate would suggest that CFPB oversight reduced lending.

The sample period is 2010 to 2013. We generally estimate the model using weighted least squares, weighting by loan amount, so that β can be interpreted as the effect on the share of dollar lending volume originated by CFPB-supervised banks. However, we also estimate unweighted regressions to study effects on the number of originated mortgages. Standard errors are clustered by county.

Results are shown in Table 1. Estimates in panel A show that CFPB oversight had little effect on total mortgage lending. The first column includes no controls; the positive coefficient indicates that the raw market share of CFPB-supervised banks increased slightly after 2011:Q2. But the coefficient drops to effectively zero in column 2, which adds census tract fixed effects, and it remains near zero after also including loan- and borrower-level controls (column 3, our preferred specification). The coefficient is quite precisely estimated; a 95 percent confidence interval implies that CFPB oversight reduces lending by no more than 1.6 percentage points, or about 4 percent of the market share of CFPB-supervised banks in our sample. Column 4 re-estimates the same model without weighting by loan amount. Here we find a small but statistically significant negative coefficient; the fraction of loans originated by CFPB-supervised banks is 1.3 percentage points lower after 2011:Q2. Jointly, columns 3 and 4 imply that CFPB-supervised banks originated relatively larger loans post-2011:Q2, as we discuss further below.

In contrast to these null results, panel B shows that CFPB oversight is associated with an economically large and statistically significant decline in FHA lending. Re-estimating the same four models using FHA mortgages only, we estimate that the formation of the CFPB reduces the lending share of affected banks by 4.5-5.5 percentage points, corresponding to 11-13 percent of the average market share of these banks. Our estimates are significant at $p < 0.01$, and robust to choices about controls and weighting.

4.1.2 Graphical representation

In Figure 4 we present "event study" plots derived from our regression models, to trace out the timing of the changes in lending. To generate the figure, we modify equation (1) by replacing the post-2011:Q2 dummy with a vector of quarterly time dummies, β_t, and then plot their evolution over time.

Panel A plots the path of β_t corresponding to column 3 of Table 1A. In the quarters around the start of CFPB operations, the estimated coefficient is near zero. The coefficient becomes more negative over the course of 2012, but the effect is not statistically significant, and it reverts toward zero over the course of 2013.

Panel B shows a very different pattern for FHA lending. There is no obvious pre-trend in the market share of the group of $>$\$10bn banks in the six quarters prior to the CFPB foundation in July 2011. But once these banks become subject to CFPB oversight, their share of FHA lending falls sharply starting in mid-2011. The estimated decrease reaches its largest magnitude in 2012:Q3 with a point estimate of about -7 percentage points, before flattening out. The trajectory of lending and the absence of a pre-trend prior to 2011:Q2 is consistent with the hypothesis that CFPB oversight is causally related to the drop in FHA lending among banks larger than \$10bn in size.

4.1.3 Robustness

We present several robustness tests in Internet Appendix B. First, we test whether our results may be an artifact of bunching by banks just below the \$10bn asset size threshold by estimating regressions excluding banks within \$2.5bn on either side of the cut-off (based on 2011:Q2 assets as before). In practice, FHA mortgage lending activity does not materially affect bank size because these loans are typically securitized rather than held in portfolio. Consequently there is no obvious reason to expect bunching considerations would be driving our results. Consistent with this intuition, excluding banks close to the \$10bn threshold has little effect on our results.

Second, we restrict our sample to purchase mortgages (i.e., excluding refinancings) in order to investigate whether the effects we identify depend on loan purpose. Third, we examine the effects of changing the asset size cutoffs for inclusion in the sample, by changing the lower bound of bank size from \$1bn to \$2.5bn, and also by changing the upper bound from \$25bn to \$50bn. These various specification changes have some effects on our point estimates, but the statistical significance of our estimates is unchanged.

4.2 Substitution effects

Next we study the composition of lending in more detail and investigate which types of lending increase to offset the drop in FHA activity. Our approach is to augment equation (1) by interacting the post-2011:Q2 dummy with indicators for different loan or borrower types, measuring whether CFPB oversight differentially affects the market segment in question. We use our preferred model which includes loan controls and census tract fixed effects, and weights observations by loan size.

Results are presented in Table 2. Column 1 interacts the post-2011:Q2 dummy with an FHA dummy. This is similar to the FHA regressions in Table 1, and in line with our earlier results, the interaction coefficient is negatively signed indicating that CFPB oversight leads to a disproportionate decline in FHA lending. The coefficient in column 1 is of similar although slightly larger magnitude than in column 3 of Table 1.

In column 2, we instead interact the post-2011:Q2 dummy with a jumbo loan indicator. Jumbos are large mortgages with a principal balance exceeding the "conforming loan limit" to be eligible for securitization through Fannie Mae and Freddie Mac. Jumbo loans are typically held in portfolio by banks, and jumbo borrowers are typically high-income households with strong credit histories. Here, we find a significant positive coefficient, meaning that banks *increased* jumbo lending in response to CFPB oversight. This shift towards larger mortgages is also apparent in the results in Table 1 which show that the dollar *value* of total lending was unaffected by CFPB oversight, but that CFPB-supervised banks made a smaller *number* of loans overall.

Column 3 examines conventional conforming loans, which make up the largest segment of the mortgage market and are usually securitized through Fannie Mae or Freddie Mac. Here, we find no differential growth in lending between CFPB-supervised and unsupervised banks.

In the final column, we interact the post-2011:Q2 dummy with a proxy for high default risk which is measured independently of the loan program. The proxy we use is a dummy equal to 1 if there is no co-applicant on the mortgage application and the ratio of the loan amount to the applicant's income exceeds 3.[14] In Internet Appendix B.2, we provide direct

[14]We use this proxy variable because HMDA data collected prior to 2018 do not include standard mortgage underwriting variables such as the borrower's credit score or the loan-to-value ratio. Servicing datasets such as Black Knight McDash contain these variables but do not identify the mortgage originator, and also have less complete coverage of the mortgage market than HMDA, particularly for smaller

evidence that this proxy is consistently associated with a higher rate of mortgage default throughout the economic cycle, based on analysis of a match between HMDA data and Black Knight McDash (hereafter "McDash") mortgage-level performance data available to Federal Reserve researchers. The higher default risk of single-applicant mortgages is also discussed by Tzioumis (2017).

We find a negative interaction effect in column 4, meaning that CFPB-supervised banks cut lending disproportionately to high credit-risk borrowers. This is in line with our FHA results, although the magnitude is considerably smaller. The difference may reflect the particular legal and regulatory risk associated with FHA lending.

To sum up, our results suggest CFPB oversight leads to a substitution away from mortgages that present elevated legal and regulatory risk, in particular FHA loans and mortgages with high credit risk. Banks substitute to large jumbo mortgages to generally high-income, creditworthy borrowers.

4.2.1 Alternative specification

As a robustness test we also analyze composition effects using an alternative difference-in-differences model where the dependent variable is a dummy equal to 1 if the loan is of a particular type (FHA, jumbo, conventional or high default risk). These outcome variables are regressed one-at-a time on a CFPB $\times$ post dummy (the coefficient of interest) as well as a set of census tract $\times$ quarter fixed effects, lender fixed effects and loan purpose dummies. The results, which are presented in Internet Appendix B.3, are similar to Table 2; namely CFPB oversight leads to a shift away from FHA lending and towards jumbo lending, with little or no significant effect on the volume of conventional lending. We do not use this approach as our main specification because it cannot easily be applied to analyze the effects of CFPB oversight on total lending, as we do in Section 4.1.[15]

lenders.

[15]In unreported results, we also conduct an analysis of the probability of loan denial, using the same difference-in-differences approach. We find no significant differential changes in denial probabilities by CFPB-supervised lenders for FHA, conventional conforming, or high-risk loans; however, we do find that they differentially lowered their probability of rejecting jumbo loan applications, in line with the lending share results discussed above.

4.3 Post-2016 election

As we have discussed, the 2016 federal election led to an expectation of a relaxation in CFPB oversight, which indeed occurred beginning in 2017 with the replacement of the Bureau's director, a sharp decline in the number of enforcement actions, and a reduction in staffing. The election thus provides a second natural experiment, this time associated with a *reduction* in the intensity of CFPB oversight. This second experiment is useful for cross-validating our earlier results, and also has the advantage of having somewhat cleaner event timing, given that the 2016 federal election results were largely unanticipated.

We use the same empirical strategy as our earlier analysis. Our sample consists of banks with assets between $1bn and $25bn as of the quarter prior to the event (2016:Q3), and we classify banks based on whether they were subject to CFPB oversight in that quarter. We then trace out differential trends in lending between the CFPB-supervised and non-supervised groups in the post-election period. Our prior, however, is that relaxing the intensity of CFPB oversight would lead to an increase in FHA lending so that the coefficient on the "post" dummy would have the opposite sign to our earlier results.

Figure 5 plots aggregate lending for the two groups of banks. In panel A, we see that total mortgage lending tracks closely for CFPB-supervised and unsupervised banks both before (2015-16) and after (2017-18) the election. On the other hand, panel B shows that FHA lending by supervised banks is visibly higher in 2017 relative to the period before the election, suggesting a potential effect consistent with our prior.

Table 3 presents results from regressions that parallel the ones estimated in Table 1 and discussed in Section 4.1.[16] In columns 1 and 2, we see that without controlling for loan characteristics, there is no significant difference in the probability of a mortgage being originated by a CFPB-supervised bank in the post-2016:Q4 period versus the quarters before.[17] Once controls are added (columns 3 and 4), there is a small positive effect of about 1 percentage point—that is, CFPB-supervised banks slightly increased their total lending relative to non-supervised banks in the period following the federal election.

The effects are substantially larger for FHA mortgages, as shown in panel B. In the specification without loan controls, the coefficient implies a +3 percentage point increase

[16] Panel B of Appendix Table IA.5 presents various robustness checks, showing that the results are qualitatively unchanged when alternative sample restrictions are imposed.

[17] The regression excludes 2016:Q4 itself, although including that quarter matters little for the results.

in the market share of CFPB-supervised lenders; this estimate increases once controls and census tract fixed effects are added, to +4.5 percentage points in column 3. Thus, these results are in line with our earlier evidence—less strict CFPB oversight leads to an increase in FHA lending.

Figure 6 plots dynamic coefficients from the regression with loan-level controls. Panel A demonstrates that for the market as a whole, there is little evidence of differential pre-trends in the quarters leading up to the 2016 election. Then, the share of mortgages originated by CFPB-supervised banks temporarily increases over 2017, although the effect seems to dissipate by late 2018.

For FHA mortgages (panel B), the post-election increase in the conditional share of lending by CFPB-supervised banks is quantitatively larger and also more persistent. The effect becomes apparent in the first quarter of 2017, and then increases in magnitude as 2017 unfolds. The effect on FHA lending volume then dissipates somewhat over 2018, although it remains economically and statistically significant. The timing of the response suggests that lenders began adjusting behavior quite quickly, and that further adjustment followed in response to changes in CFPB supervision and enforcement practices.

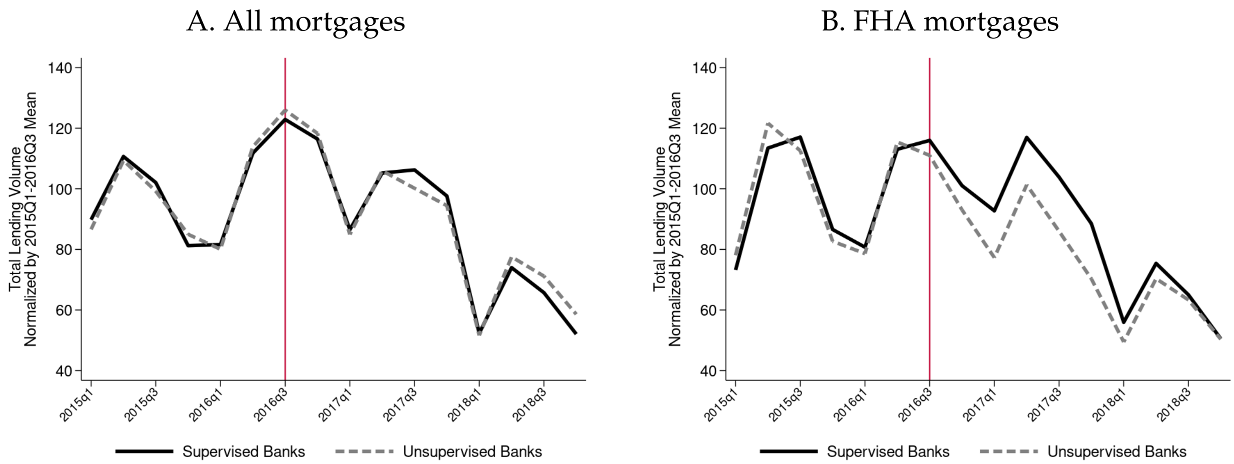
We also note from the graph that, although the FHA lending share of CFPB-supervised banks was roughly constant over 2015:Q3 to 2016:Q4, it had substantially increased during the first two quarters of 2015. Since those quarters are included as part of the "pre" period in our regressions in Table 3B, this increase will affect the estimated overall effect reported in the table. However, if we drop these two quarters from the regression, the magnitude of the coefficients only decreases by about 20%.

In summary, results from this second natural experiment around the 2016 federal election are consistent with our earlier findings. In both cases our estimates imply that stricter CFPB oversight is associated with a reduction in FHA lending activity, but at most leads to only a small reduction in total lending.

4.4 Assessing alternative explanations

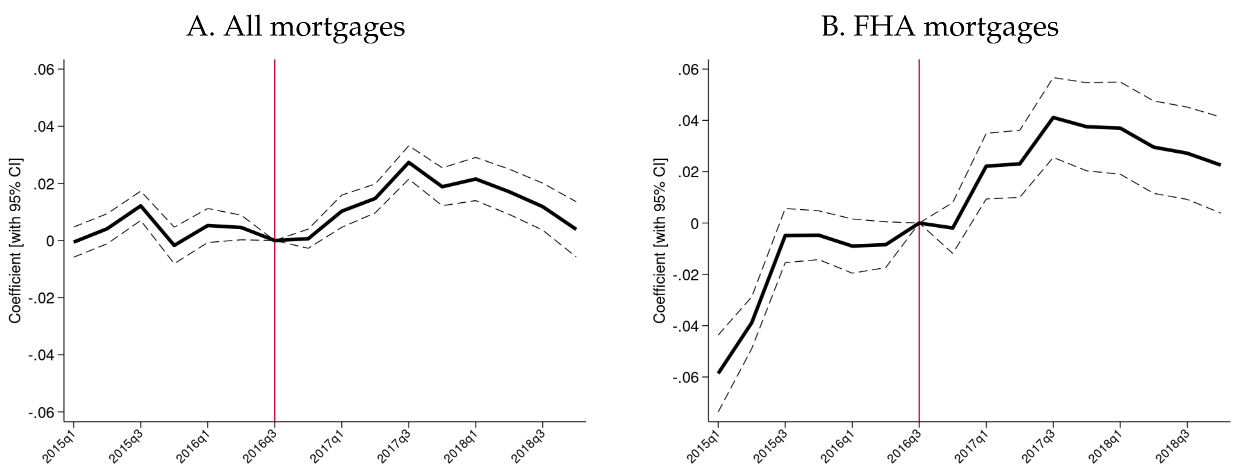
Although we attribute our results to the CFPB's supervision and enforcement activities, CFPB oversight is not the only regulatory implication of crossing the $10bn asset size threshold under the Dodd-Frank Act. Specifically, banks and/or bank holding companies above $10bn in size are required to: (i) run internal company-run stress tests of their per-

Figure 5: Total mortgage originations of CFPB-supervised and CFPB-unsupervised banks around the 2016 federal election.



Note: Sample includes banks with assets between $1bn and $25bn as of 2016:Q3. Total originations of each group are normalized by the average volume over 2015:Q1-2016:Q3.

Figure 6: Evolution of probability that a mortgage was originated by a CFPB-supervised bank around the 2016 federal election.



Note: Quarterly series show estimated quarterly coefficient (relative to 2016:Q3) and 95% confidence intervals. Regressions also control for census tract fixed effects and loan-level controls, and observations are weighted by loan amount.

formance under adverse economic scenarios, (ii) establish a risk committee, (iii) comply with a more stringent Volcker Rule, and (iv) comply with caps on debit card interchange fees.[18] Research has found evidence of either slower growth or a bunching in the bank size distribution below the $10bn asset size threshold after the passage of Dodd-Frank (Morgan and Yang, 2016; Bouwman et al., 2018; Alvero et al., 2020), presumably reflecting the combined effects of these different regulations.[19]

Could our results be due to the influence of these other regulations? Our interpretation is that the influence of the CFPB is the most plausible mechanism, given that these other regulations do not directly focus on mortgage lending or other types of consumer finance. The timing of changes in bank mortgage lending behavior also seems to support this interpretation; we observe changes in behavior shortly after the formation of the CFPB in July 2011, whereas for example the final rule related to stress testing of banking institutions with assets of $10-50bn was not issued until October 2012, and banks in the $10bn-$50bn size range were not required to form a risk committee until July 2015. Balasubramanyan et al. (2019) also find no evidence that forming a risk committee has a causal effect on bank risk-taking.

As an additional check, we conduct a placebo test to examine whether small business lending, another form of risky lending but one which is not part of the CFPB's remit, responds to the formation of the CFPB in 2011 or the regime change after 2016. This analysis is discussed in detail in Section 7.1. We follow the same empirical strategy, comparing the differential path of lending for banks subject to CFPB oversight versus otherwise similar banks below the $10bn asset size cutoff. We do not find evidence of lending responses in the small-business market consistent with those observed for FHA mortgage lending, implying that our findings reflect factors specific to mortgage lending rather than general effects of heightened regulation on risk-taking and credit provision.

5 Mortgage delinquency effects

We next examine the effects of CFPB oversight on borrower delinquency and default, considering two channels for such effects: (1) oversight may lead lenders to tighten their

[18] Kay et al. (2018) find that noninterest income fell for banks subject to the debit interchange fee cap, but that these banks offset most of the lost interchange income through increases in deposit fees (see also Mukharlyamov and Sarin, 2019). Note that the asset size threshold for some of these requirements was lifted in May 2018 under the Economic Growth, Regulatory Relief and Consumer Protection Act.

[19] Alvero et al. (2020) also use a structural approach to estimate the costs of these regulations, and conclude that these costs are relatively low.

ex-ante lending standards, either based on hard information like credit scores or loan-to-value ratios, or based on soft information. Indeed, we have already shown that CFPB-supervised lenders reduced lending to FHA borrowers and borrowers with a high loan-to-income ratio and no co-applicant. (2) lenders may take ex-post actions to mitigate defaults through the way they service the mortgage. Servicing involves collecting borrower payments and forwarding them to relevant parties (investors, tax authorities, and insurers). But it also involves working with delinquent borrowers to help them become current again and to avoid foreclosure. This can involve credit counselling, payment plans, loan modifications, or potentially working out a short sale.[20]

The servicing channel is of particular interest given that mortgage servicing practices have been a major focus of CFPB rule-making and enforcement.[21] It thus seems plausible that financial institutions subject to CFPB scrutiny may have adopted more "borrower-friendly" servicing practices as a result, or invested more in improving the quality of their servicing systems.

Since HMDA data do not contain loan performance information, we supplement HMDA with a proprietary loan-level data set containing default outcomes and loan characteristics for FHA loans, previously used in Bhutta and Hizmo (2020).[22] The data set contains loan characteristics of each mortgage insured by the FHA over the years 2009-2013 along with the exact origination date and lender identifiers. We also observe for each loan whether it became 30, 60 or 90 days delinquent between the time of origination and the end of 2016, and if so, in which month each delinquency state first occurred.

As in our earlier analysis, we restrict the sample to banks with \$1bn-\$25bn in assets as of June 30, 2011. We estimate loan-level linear probability regressions of the form:

$$Y_{ijct} = \alpha_j + \gamma_t + \nu_{ct} + \beta(CFPBsupervised_j \times post2011Q2_t) + \theta X_i + \varepsilon_{ijct}, \qquad (2)$$

[20]For an introduction to servicing, see e.g. https://www.urban.org/policy-centers/housing-finance-policy-center/projects/mortgage-servicing-collaborative/help-me-understand-mortgage-servicing/what-mortgage-servicing. Agarwal et al. (2017) present evidence that variation in servicing practices across financial institutions is important for understanding foreclosure outcomes during the Great Recession.

[21]For examples of CFPB enforcement actions related to servicing, see https://www.housingwire.com/articles/31517-cfpb-fines-flagstar-375-million-for-mortgage-servicing-violations/ and https://www.housingwire.com/articles/33627-cfpb-and-ftc-fine-green-tree-63-million-for-mistreating-borrowers/.

[22]We are very grateful to Neil Bhutta for providing access to the data set and helping us with the analysis in this section.

where Y_{ijct} is a delinquency outcome (e.g., 90+ days delinquent) for a loan i originated by lender j in county c and month t; α_j and γ_t are lender and origination-month fixed effects; and ν_{ct} are county-by-origination-year fixed effects. Our main coefficient of interest is β, which measures the differential probability of the delinquency outcome occurring if the loan was originated by a CFPB-supervised lender after mid-2011. Finally, in some specifications we add detailed loan-level characteristics X_i to control for the effect of observable risk factors aside from location.

For our delinquency outcomes, we consider whether a loan ever reaches 30, 60, or 90+ day delinquency (meaning that the borrower is behind by 1, 2, or 3 monthly payments). We furthermore study conditional delinquency transitions, namely whether a loan that reached 30-day (or 60-day) delinquency, subsequently reached 60-day (or 90-day) delinquency. These transitions are more plausibly affected by servicing actions, given that the servicer generally only intervenes once the loan has already become delinquent. In these transition regressions, we control for the month in which the loan reached the delinquency state we are conditioning on.

The data set contains just over 365 thousand loans, of which 23.4% become 30 days delinquent at least once, 13.3% become at least 60 days delinquent, and 9.7% become 90 days delinquent.[23] We emphasize that these delinquency rates are much higher than for the mortgage market as a whole for these origination cohorts, due to the high default risk of FHA loans compared to typical prime mortgages.

Results are presented in Table 4. Columns 1 and 2 report the estimated effect of CFPB oversight on the probability of loans becoming at least one month delinquent (measured by the coefficient on CFPB $\times$ post). Column 1 includes no loan controls, while column 2 controls for quadratic functions of credit score and the loan-to-income ratio, loan-to-value, loan type and other controls (see table notes for full list). We see that in both columns, the estimated effect is positive and at least marginally significant, meaning that CFPB-supervised banks if anything tended to originate slightly *riskier* FHA loans in the post-period according to this metric.

In columns 3 to 6, however, we see that mortgages originated by CFPB-supervised lenders in the post-2011:Q2 period are less likely to transition from one stage of delin-

[23] In the regressions, the sample size is slightly smaller, because "singleton" observations (completely determined by the combination of fixed effects) are dropped. Furthermore, when we add loan-level characteristics as controls, we lose a few more observations, due to missing data.

quency to the next, more serious stage. For the 60-days delinquent to 90+ days delinquent transition probability, this effect is statistically significant at $p < 0.05$ without loan controls, and at $p < 0.01$ with controls.[24] The coefficient of -0.0426 for the specification including controls is equivalent to a 16% increase in the share of 60-day delinquent loans that avoid progressing to 90+ days delinquency. (This is because in our sample, only 27% of mortgages that become 60 days delinquent avoid progressing to 90+ days delinquency.)

These results are consistent with the hypothesis that CFPB oversight has induced servicers to expend more effort on outreach and delinquency mitigation for struggling borrowers, perhaps due to the implicit threat of legal action. It is not the case, however, that FHA loans from CFPB-supervised lenders are less risky ex-ante, either on observable or unobservable dimensions. Taken together with our earlier results, it seems lenders reduced risk on the extensive margin by originating fewer FHA loans, rather than adjusting on the intensive margin.

6 Regulatory arbitrage and nonbank substitution

Small banks' exemption from CFPB oversight creates a potential opportunity for regulatory arbitrage. Nonbank entities fall under the CFPB's purview regardless of size or whether they are affiliated with a CFPB-exempt bank. So small banking organizations may find it advantageous to shift lending from nonbank mortgage subsidiaries to their commercial bank in order to avoid CFPB scrutiny.[25] Demyanyk and Loutskina (2016) find evidence of this kind of within-BHC regulatory arbitrage prior to the 2008 financial crisis, although in the opposite direction due to lax regulation of BHCs' nonbank mortgage subsidiaries.

To test for the presence of regulatory arbitrage during our post-crisis sample period,

[24] We do not report the regressions where we directly use 60-day and 90-day delinquency as the dependent variable. The coefficient of interest is slightly positive (negative) of 60-day (90-day) delinquency, but not close to statistically significant in either case. This reflects the offsetting effects of the higher transition into 30-day delinquency for CFPB-supervised banks and the lower transition from 30-day delinquency into 60-day and particularly 90+ day delinquency.

[25] The term "banking organization" here refers to either: i) a bank holding company (BHC) that controls one or more banks and may also control a number of nonbank subsidiaries, or ii) a standalone high-holder commercial or savings bank which may have bank or nonbank subsidiaries but is not organized as a bank holding company. Complex banking organizations are typically organized as BHCs, but not always. See Avraham, Selvaggi, and Vickery (2012) for more institutional background.

we expand our mortgage sample to include loans originated by nonbank affilates of the set of banks studied in our main analysis. We then estimate whether the share of bank lending (*vis-à vis* lending by affilated nonbanks) increases after 2011:Q2 in banking organizations where the bank subsidiaries are exempt from CFPB oversight.

We use data from the National Information Center and the Avery file to identify the presence and identity of a bank or BHC high-holder for each nonbank lender in HMDA. We then estimate variants of the following linear probability model:

$$bankloan_{icbt} = \alpha_t + \kappa_c + \theta_b + \beta \cdot (post2011Q2_t \times CFPBexempt_b) + \Gamma X_{ict} + \varepsilon_{ict}, \quad (3)$$

where $bankloan_{icbt}$ is a dummy equal to 1 if mortgage i in census tract c originated in quarter t by banking organization b was originated by a commercial or savings bank entity rather than a nonbank mortgage company affiliate; α_t, κ_c and θ_b are time, census tract, and banking organization fixed effects (the most stringent specification uses census-tract-by-quarter fixed effects, α_{ct}); $post2011Q2_t$ is a dummy equal to 1 during the time period when the CFPB is active; $CFPBexempt_b$ is a dummy equal to one for banking organizations in which the bank subsidiaries remain exempt from CFPB oversight; and X_{ict} is a set of loan characteristics included in some specifications.

β is the coefficient of interest. A positive estimate would be consistent with the presence of regulatory arbitrage: the entry of the CFPB induced small banking organizations to shift lending activity from their nonbank to bank affiliates.

Results are presented in Table 5. In keeping with our earlier analysis, panel A analyzes the full sample while panel B studies the subsample of FHA mortgages. We consider four specifications including different combinations of controls; starting from from column 1, which includes only quarterly time fixed effects, to column 4 which includes census tract $\times$ quarter fixed effects, banking organization fixed effects, and a set of loan controls including race, gender, log loan amount and log income (the same controls used in our earlier analysis).

Estimates based on the full loan sample reveal little evidence of any shift in lending driven by regulatory arbitrage. The coefficients are positively signed, consistent with the hypothesis, but the estimates are economically small, and statistically insignificant in three of the four columns.

For FHA loans, however, there is much stronger evidence in favor of the regulatory arbitrage hypothesis (panel B). The estimate in column 1 implies that the raw fraction of FHA lending being routed through a banking subsidiary increases by 9.1 percentage points after 2011:Q2 among banking organizations where the banks are exempt from CFPB oversight. The estimate is statistically significant at the 1 percent level. 30% of FHA lending is done through nonbank subsidiaries in the sample as a whole, our sample, so the coefficient represents around a one-third drop in nonbank lending relative to this sample average.

Columns 2 through 4 help unpack the channels through which this substitution in FHA lending from nonbank to bank entities occurs. The coefficient drops by about a quarter, from 0.091 to 0.070, when we include loan controls and census tract fixed effects (column 2), reflecting differences in the geographic footprint and composition of FHA lending between banks and nonbanks. The coefficient then drops to 0.031 when we include lender fixed effects, indicating that part of the substitution occurs *across* banking organizations—that is, organizations with a lower pre-existing share of nonbank lending experienced comparatively higher FHA lending growth. The fact that the column 3 coefficient is still economically and statistically significant shows however that part of the effect occurs within banking organizations (e.g., some BHCs downsized their nonbank mortgage company and instead lent through their commercial bank). Finally the coefficient is similar although slightly smaller when we replace the time and census tract fixed effects with a more saturated set of census tract $\times$ time fixed effects.

In Section B.4 of the Internet Appendix, we also investigate whether this reshuffling of lending between nonbank and bank affiliates is a significant driver of our earlier results on bank lending volume. Specifically, we re-estimate Table 1 and Figure 4 retaining both bank and nonbank affiliates in the loan-level sample (rather than dropping nonbanks), and test whether CFPB oversight of bank subsidiaries affects *total* lending by the banking organization. We find economically and statistically significant effects in the same direction as in our primary analysis, although the estimates are smaller in magnitude—the effect size of CFPB oversight on the FHA lending market share drops from 13% of the sample mean to 8%. This attenuation of the effect size in part reflects a dilution of the treatment given that there is no regulatory cutoff at $10bn applying to nonbanks, and in part reflects the nonbank-bank substitution discussed above. Our main conclusions are unchanged, however, and the timing of the drop in FHA lending is similar to 4 (see figure in Section B.4 of the appendix).

To sum up, the estimates in Table 5 show that lenders shift FHA lending away from the entities subject to CFPB oversight towards those which are not, consistent with earlier evidence in Demyanyk and Loutskina (2016) on within-BHC regulatory arbitrage in mortgage lending behavior. Our results contribute to research on how regulatory arbitrage shapes financial institution behavior (e.g., see Acharya et al., 2013 and Koijen and Yogo, 2016 for two recent contributions), and highlight the potentially unintended consequences of applying different regulations across entities within a financial firm.

6.1 Substitution to independent nonbanks

In related analysis, we also examine whether CFPB oversight is associated with a substitution in lending towards *independent* nonbanks (that is, monoline nonbank mortgage lenders not affiliated with a banking organization). These independent mortgage lenders grew rapidly during our sample period, particularly in the FHA market. As already noted above, nonbanks are also subject to CFPB oversight, independent of their size. However, they may be less sensitive to the associated legal risks than banks, which arguably have higher "franchise value."

In Internet Appendix C, we present the results of a matched-sample analysis to test whether nonbanks gained FHA market shares disproportionately from CFPB-supervised banks in the post-2011:Q2 period. For each bank we find a matching nonbank with similar lending volumes and growth in the year prior to 2011:Q2. We then study whether the probability an FHA loan was originated by the sample of matched nonbanks increases post-2011:Q2, and in particular whether this shift is more pronounced for the subset of banks that become subject to CFPB oversight.

We find mixed evidence that nonbanks' growth accelerated differentially relative to CFPB-supervised banks. Results for some specifications are in line with the hypothesis, but the economic magnitude and statistical significance is sensitive to which cutoff rule we use for match quality and to which set of controls and fixed effects are included. And even in the specifications where we do find statistical significance, the magnitudes are significantly smaller than the overall growth in nonbank FHA lending during the post-crisis period.

This analysis suggests that factors other than the formation of the CFPB are quantitatively more important in accounting for the rise of nonbanks, such as changes in capital and liquidity regulation, banks' overall aversion to the legal risk associated with FHA lending, or technological progress (e.g., Buchak et al., 2018; Kim et al., 2018; Fuster et al., 2019; Gete and Reher, 2020). We are reluctant to draw emphatic conclusions though, given the sensitivity of our results and the inherent limitations of the matching exercise.

7 Additional analysis and caveats

This section presents additional analysis examining other effects of CFPB oversight, including a placebo test of its effects on small business lending, effects on the size distribution of banks, and effects on balance sheet and income statement outcomes. We also discuss some issues of interpretation and caveats related to our findings.

7.1 Small business lending

First, we investigate whether our results are specific to consumer lending, or instead reflect a general contraction in risky lending across the bank's loan portfolio. This is done by comparing our FHA results to a placebo test on a category of non-consumer loans, namely small business loans. Small business loans are an appropriate benchmark for FHA mortgage lending for several reasons: the loans are small (less than \$1m), they are typically risky, and they are highly localized. However, they are not subject to CFPB oversight, nor are they associated with elevated legal risk.

If our mortgage lending results reflect bank-wide changes in risk management or risk-tolerance, we would expect to also find evidence of a contraction in risky small business lending. The absence of such a pattern, in contrast, supports the view that our results are due to factors specific to consumer lending—in particular the intensity of CFPB oversight—rather than other regulations imposed above the \$10bn asset size threshold.

We obtain data on small business lending (SBL) originations from the FFIEC Community Reinvestment Act (CRA) disclosures. The CRA data contains annual small business lending originations by bank at the county level. The data is collected for banks above a specific size cutoff that is indexed to inflation. The cutoff during the period of our analysis is roughly \$1.2bn.

We follow a similar empirical strategy to our earlier analysis, but with some modifications to account for the fact that the SBL data is collected at an annual frequency, contains a smaller sample of banks and is less granular than the HMDA data.

For both FHA mortgages and SBL originations, we estimate the market share of lending originated by CFPB-supervised banks using a county-level panel regression:

$$CFPBLoanShare_{lt} = \alpha_l + \beta \cdot Post_t + \varepsilon_{lt}. \quad (4)$$

The dependent variable is the share of loans in county l and year t issued by CFPB-supervised banks. The regression also includes county fixed effects. Shares are calculated as a fraction of total originations among a balanced panel of banks with assets between \$1bn and \$25bn, where the lower bound is ultimately determined by the CRA small business lending reporting requirements. All regressions are weighted by overall lending in the county to ensure small counties are not driving the results. In an effort to ensure comparability across the analyses, we restrict the sample to common counties.

We use the regression to estimate the response of the market share of CFPB-supervised banks to the creation of the CFPB and to the 2016 election. The coefficient on the $Post_t$ indicator estimates the change in lending share in response to the formation of the CFPB (2012 onward) or post-election changes in the regulatory environment (2017 onward). For the analysis around the creation of CFPB the sample period is the years 2010 through 2013, and for the election it is the years 2015 through 2018. In both cases, the sample includes two years of post-event data and two years of pre-event data. Standard errors are clustered by county. An important feature of our analysis is that we estimate the same econometric model for FHA mortgages and small business loans, ensuring that we can compare the coefficients on an apples-to-apples basis.

Regression results are reported in Table 6. Panel A contains the results for FHA mortgages and panel B the results for small business loans. For FHA mortgages, the results roughly mirror the findings of Tables 1 and 3, panel B. Both the number of loans, column 1, and the dollar volume, column 2, specifications estimate a decline in FHA market share for CFPB-supervised banks after 2011. The estimated response to the 2016 election in columns 3 and 4 is positive and statistically different from zero at the 5 and 10 percent significance levels. Relative to the loan-level results, the signs and overall magnitudes are consistent, although the estimated effects in response to the 2016 federal election are slightly smaller.

The small business lending results in panel B, however, reveal starkly different responses to these two events. With respect to the creation of the CFPB, the regressions reveal a statistically significant positive response (1-2.6 percentage points). This positive response contrasts with the negative response by FHA mortgages, of approximately -4 to -5 percentage points. The post-election outcomes are also striking. In both specifications, columns 3 and 4, the market share of CFPB-supervised banks *declines* 2 to 4 percentage points following the election. In contrast, the CFPB market share of FHA lending increases 1-2 percentage points, in keeping with our earlier results that CFPB-supervised banks expanded FHA lending in response to a relaxation of oversight.

These stark differences between the SBL and FHA results show that the patterns we found for mortgages are not generally true of other types of risky, local lending. If anything they suggest that affected banks substituted towards small business lending in response to tighter regulatory oversight of consumer lending. The results are consistent with the interpretation that our findings for the mortgage market reflect the causal effect of CFPB oversight on mortgage lending behavior, rather than other factors that led to bank-wide changes in lending policies.

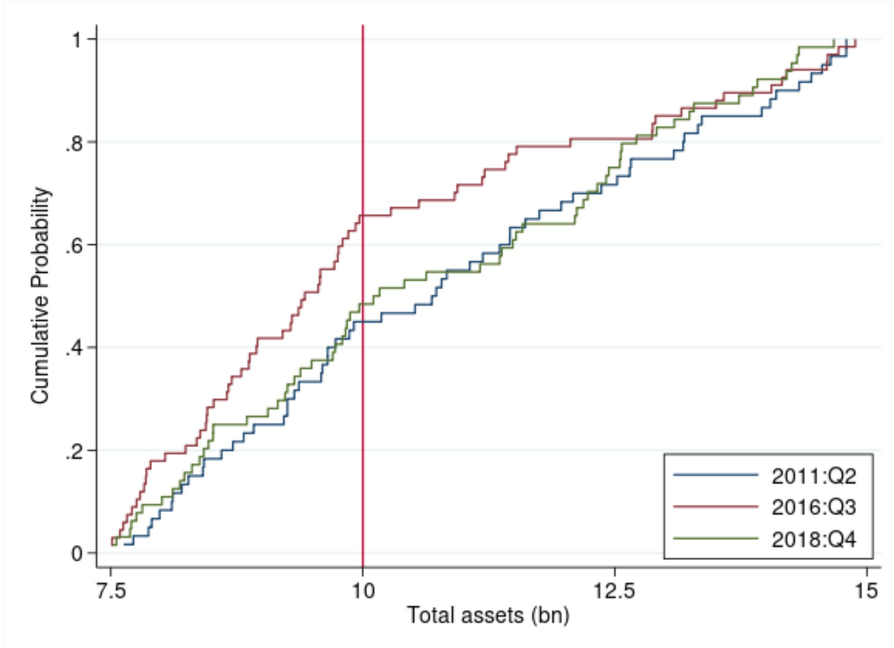
7.2 Bunching and the bank size distribution

Our earlier histograms (Figure 2) suggest that there is significant bunching just below the $10bn threshold in 2016:Q3, which is not present in 2011:Q2 just prior to the formation of the CFPB. In Figure 7 we investigate this further by plotting the cumulative density function (CDF) of firm size in the region near $10bn in 2011:Q2, 2016:Q3, and at the end of our post-election sample period in 2018:Q4.

Consistent with our histograms, and with Morgan and Yang (2016), the CDF for 2016:Q3 is kinked in the neighborhood around $10bn, reflecting asset size bunching below the regulatory threshold. As we discussed in Section 4.1, there is no reason to expect this bunching drives our empirical results, particularly since our results hold if we exclude banks close to the threshold.

More interestingly, however, we find that the kink in the size distribution has disappeared by 2018:Q4, when the distribution looks similar 2011:Q2. A Kolmogorov-Smirnov test rejects the null that the 2016:Q3 distribution is the same as the 2011:Q2 distribution at the 5% level, and rejects the null that it is the same as the 2018:Q4 distribution at the 10%

Figure 7: Cumulative density function of bank size around $10bn size threshold



Note: Cumulative distribution function, based on the population of commercial banks and savings banks drawn from the Call Reports and Thrift Financial Reports. P-values from two-sided Kolmogorov-Smirov tests of equality of distributions are 0.048 (2016:Q3 vs. 2011:Q2) and 0.092 (2016:Q3 vs. 2018:Q4).

level.

To our knowledge, the literature has not previously highlighted this "de-kinking" of the bank size distribution between 2016 and 2018. This result supports the narrative that the perceived costs of bank regulation declined after 2016:Q3, consistent with our empirical analysis of this period. We emphasize though that here we cannot disentangle the effects of the CFPB from other Dodd-Frank regulations, since this analysis looks more generally at the bank size distribution rather than focusing on consumer lending activity.

7.3 Effects on balance sheet and income statement outcomes

In a related analysis reported in Internet Appendix D, we use quarterly Call Report filings to investigate whether the $10bn threshold is associated with broader changes in bank financial outcomes after 2011:Q2. We find no statistically significant evidence of such effects in terms of balance sheet composition, profitability, or different components of income. However, the statistical power of the analysis is limited, making it difficult to draw strong conclusions.[26]

7.4 Caveats and interpretation

Although the robustness tests and other exercises presented above are effective at ruling out most alternative interpretations for our results, it is important to emphasize a number of more general caveats on the interpretation of our findings. First, we focus on banks in the neighborhood of the \$10bn asset size threshold, and as a result the effects we estimate are "local" effects for this size group of banks. The effects of CFPB oversight on lending and servicing behavior could be either larger or smaller than our estimates for larger banks which make up a more significant share of total mortgage lending.[27]

Second, we emphasize that our study focuses only on a subset of the potential effects of the supervision and enforcement activities of the CFPB. In particular, in addition to potential effects on mortgage delinquency outcomes that we do study, there may be other potential benefits for consumers of heightened regulatory oversight (e.g., a reduction in predatory lending or other abusive practices). Thus, while our study adds to the body of knowledge about the CFPB, our results do not speak to the overall net social benefits or costs of the regulator.

Finally, we note that the CFPB has also engaged in significant rule-writing activities since its creation (e.g., the TILA-RESPA integrated disclosure rule, also known as TRID, or the qualified mortgage requirements), which apply to all lenders. Our results do not speak to the effects of these rules.

8 Conclusion

In this paper, we find that the CFPB's examination, supervision and enforcement activities have significant effects on bank credit supply and other aspects of bank behavior. In

[26] In some analyses, we allow for differential effects of CFPB oversight depending on whether the bank focuses on consumer lending. Although we find no effects of CFPB oversight in these regressions, we do find some evidence for higher expense ratios of high-consumer-lending banks in the post-period, independently of whether they are CFPB-supervised or not. This is consistent with other evidence pointing toward increasing costs of mortgage lending after the financial crisis (D'Acunto and Rossi, 2020; Fuster et al., 2017).

[27] For example, our results may understate the overall effect if the CFPB disproportionately targets supervision and enforcement efforts towards the largest banks. On the other hand, larger institutions may be able to more effectively manage the compliance costs of CFPB supervision due to scale economies.

particular, the formation of the CFPB in 2011 led CFPB-supervised banks to withdraw from risky FHA lending, a market where borrowers are typically lower-income and where lending presents elevated legal and regulatory risk. FHA lending by CFPB-supervised banks subsequently rebounded following a relaxation in oversight after the 2016 federal election. We also document shifts in FHA lending within banking organizations to bank entities that are exempt from CFPB oversight, as an apparent form of regulatory arbitrage.

Balanced against these effects on credit supply, we also find evidence suggesting that CFPB oversight is associated with an improvement in mortgage servicing practices, leading FHA mortgages from CFPB-supervised banks to become less likely to transition from moderate to serious delinquency. Poor servicing practices were an important driver of the foreclosure crisis during the Great Recession; our results suggest that tighter regulatory oversight may help reduce inefficient foreclosures during future downturns.

While we are not able to make an overall determination of the welfare benefits of dedicated consumer protection oversight, our results suggest that there is a trade-off between more intense oversight that is intended to protect vulnerable borrowers and the willingness on the part of supervised banks to lend to these borrowers. Careful consideration of these trade-offs is important when crafting regulations and supervisory mechanisms to promote consumer financial protection.

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Table 1: Regression of probability of loan being originated by CFPB-supervised bank on post-2011:Q2 dummy and controls.

A. All mortgages

	(1)	(2)	(3)	(4)
Post-2011Q2	0.0230**	-0.00172	-0.00289	-0.0131***
	(0.00974)	(0.00731)	(0.00688)	(0.00432)
N	3704987	3702041	3702041	3702041
Mean Y	0.38	0.38	0.38	0.33
Loan controls	N	N	Y	Y
Census Tr. FE	N	Y	Y	Y
Weighted	Y	Y	Y	N

B. FHA mortgages

	(1)	(2)	(3)	(4)
Post-2011Q2	-0.0446***	-0.0479***	-0.0546***	-0.0478***
	(0.00770)	(0.00582)	(0.00617)	(0.00578)
N	356325	341892	341892	341892
Mean Y	0.41	0.41	0.41	0.39
Loan controls	N	N	Y	Y
Census Tr. FE	N	Y	Y	Y
Weighted	Y	Y	Y	N

Dependent variable: Dummy = 1 if loan was originated by a bank that became subject to CFPB oversight after 2011:Q2. Sample includes banks with $1bn-25bn in assets as of 2011:Q2. Sample period is 2010:Q1 to 2013:Q4. Only mortgages with loan amount up to $5 million are included. Loan controls include log(applicant income), log(loan amount), and indicator variables for applicant race and gender, whether the property is owner-occupied, loan purpose, loan type (conventional, FHA, VA, FSA), jumbo status, missing applicant income, and whether there is a co-applicant. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Table 2: Regression of probability of loan being originated by CFPB-supervised bank on post-2011:Q2 dummy interacted with loan characteristics.

	(1)	(2)	(3)	(4)
Post-2011Q2	0.00282	-0.00997	-0.000147	-0.00164
	(0.00718)	(0.00701)	(0.00789)	(0.00669)
Post-2011Q2 × FHA	-0.0647***			
	(0.00745)			
Post-2011Q2 × Jumbo		0.0536***		
		(0.0110)		
Post-2011Q2 × Conv. Conforming			-0.00350	
			(0.00763)	
Post-2011Q2 × (No Coapp. & High LTI)				-0.00784**
				(0.00352)
N	3702041	3702041	3702041	3702041
Mean Y	0.38	0.38	0.38	0.38
Loan controls	Y	Y	Y	Y
Census Tr. FE	Y	Y	Y	Y
Weighted	Y	Y	Y	Y

Dependent variable: Dummy = 1 if loan was originated by a bank that became subject to CFPB oversight after 2011:Q2. Sample includes banks with $1bn-25bn in assets as of 2011:Q2. Sample period is 2010:Q1 to 2013:Q4. Only mortgages with loan amount up to $5 million are included. Loan controls include log(applicant income), log(loan amount), and indicator variables for applicant race and gender, whether the property is owner-occupied, loan purpose, loan type (conventional, FHA, VA, FSA), jumbo status, missing applicant income, and whether there is a co-applicant. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Table 3: Regression of probability of loan being originated by CFPB-supervised bank on post-2016:Q4 dummy and controls.

A. All mortgages

	(1)	(2)	(3)	(4)
Post-2016Q4	-0.00158	0.000509	0.0128***	0.00891***
	(0.00467)	(0.00302)	(0.00295)	(0.00242)
N	2792412	2790772	2784815	2784815
Mean Y	0.31	0.31	0.31	0.30
Loan controls	N	N	Y	Y
Census Tr. FE	N	Y	Y	Y
Weighted	Y	Y	Y	N

B. FHA mortgages

	(1)	(2)	(3)	(4)
Post-2016Q4	0.0312***	0.0300***	0.0452***	0.0393***
	(0.00889)	(0.00689)	(0.00656)	(0.00526)
N	288948	276869	275662	275662
Mean Y	0.38	0.37	0.37	0.37
Loan controls	N	N	Y	Y
Census Tr. FE	N	Y	Y	Y
Weighted	Y	Y	Y	N

Dependent variable: Dummy = 1 if loan was originated by a bank that was subject to CFPB oversight as of 2016:Q3. Sample includes banks with $1bn-$25bn in assets as of 2016:Q3. Sample period is 2015:Q1 to 2018:Q4. Only mortgages with loan amount up to $5 million are included. Loan controls include log(applicant income), log(loan amount), and indicator variables for applicant race and gender, whether the property is owner-occupied, loan purpose, loan type (conventional, FHA, VA, FSA), jumbo status, missing applicant income, and whether there is a co-applicant. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Table 4: CFPB oversight and delinquency outcomes for FHA mortgages

	(1)	(2)	(3)	(4)	(5)	(6)
	30-day delinquency		30-to-60 transition		60-to-90+ transition	
CFPB-sup. × Post-2011Q2	0.0079**	0.0077*	-0.0051	-0.0062	-0.0357**	-0.0426***
	(0.0039)	(0.0041)	(0.0147)	(0.0116)	(0.0157)	(0.0153)
N	363,512	347,014	82,920	79,703	46,280	44,456
Loan characteristics		Y		Y		Y
Bank fixed effects	Y	Y	Y	Y	Y	Y
Origination Month FE	Y	Y	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y	Y	Y
Delinquency Month FE			Y	Y	Y	Y

FHA loan-level regression of different delinquency outcomes (0/1) on indicator for whether bank that originated the loan is subject to CFPB oversight after 2011:Q2 onward interacted with indicator for whether the loan was originated after 2011:Q2. Sample includes banks with $1bn-$25bn in assets as of 2011:Q2. Loan characteristics that regressions in even columns control for include quadratic function of credit score, quadratic function of loan-to-income ratio, back-end DTI, LTV, log(loan amount), and fixed effects for property type, loan type (purch, refi, streamline refi, cashout), first-time homebuyer, ARM, and 15-year term. Standard errors reported in parentheses are clustered by lender. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Table 5: Substitution of lending across affiliates to avoid CFPB oversight

Dependent variable = 1 if loan was originated by a bank rather than a nonbank affiliate

A. All mortgages

	(1)	(2)	(3)	(4)
non-CFPB × Post-2011Q2	0.00986	0.0183**	0.00923	0.0156
	(0.00997)	(0.00891)	(0.00901)	(0.0146)
N	4313817	4311328	4311328	4128054
Mean Y	0.86	0.86	0.86	0.86
Quarter FE	Y	Y	Y	N
Census tract FE	N	Y	Y	N
Loan controls	N	Y	Y	Y
Banking org. FE	N	N	Y	Y
Census tract × quarter FE	N	N	N	Y

B. FHA mortgages

	(1)	(2)	(3)	(4)
non-CFPB × Post-2011Q2	0.0909***	0.0698***	0.0310***	0.0256***
	(0.0148)	(0.0109)	(0.00919)	(0.00908)
N	509608	497118	497102	324301
Mean Y	0.70	0.70	0.70	0.68
Quarter FE	Y	Y	Y	N
Census tract FE	N	Y	Y	N
Loan controls	N	Y	Y	Y
Banking org. FE	N	N	Y	Y
Census tract × quarter FE	N	N	N	Y

Includes banks with $1bn-25bn in assets as of 2011:Q2. Loan-level sample includes mortgages originated by these banks as well as any of their nonbank affiliates. Sample period is 2010:Q1 to 2013:Q4. Only mortgages with loan amount up to $5 million are included. Loan controls include log(applicant income), log(loan amount), and indicator variables for applicant race and gender, whether the property is owner-occupied, loan purpose, loan type (conventional, FHA, VA, FSA), jumbo status, missing applicant income, and whether there is a co-applicant. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Table 6: County-level regressions of CFPB-supervised market share for FHA and small business loans

A. FHA mortgages

	CFPB Creation		Election	
	(1)	(2)	(3)	(4)
Post	-0.0441***	-0.0467***	0.0150**	0.0130*
	(0.0064)	(0.0063)	(0.0067)	(0.0067)
Observations	6741	6741	7035	7035
Adjusted R^2	0.885	0.879	0.866	0.861
CFPB Share	Loans	$ Vol.	Loans	$ Vol.
County FE	Y	Y	Y	Y
Weighted	Y	Y	Y	Y

B. Small business loans

	CFPB Creation		Election	
	(1)	(2)	(3)	(4)
Post	0.0259***	0.0131**	-0.0399***	-0.0222***
	(0.0039)	(0.0054)	(0.0053)	(0.0045)
Observations	7079	7079	7166	7166
Adjusted R^2	0.951	0.941	0.910	0.936
CFPB Share	Loans	$ Vol.	Loans	$ Vol.
County FE	Y	Y	Y	Y
Weighted	Y	Y	Y	Y

County-level regression of annual CFPB-supervised market share in response to CFPB creation and the 2016 election. Market shares are based on number of loans (columns 1 and 3) and dollar volume (columns 2 and 4). Market shares for FHA mortgages, panel A, and small business loans (SBLs), panel B, are calculated using a balanced panel of banks over the event windows. Banks are restricted to those that report small business lending to the CRA ($>\sim$ $1.2bn in assets) and with less than $25bn in assets. The sample windows for CFPB creation is 2010 to 2013 and the event period is post-2011. The sample window for the 2016 election is 2015 to 2018 and the event period is post-2016. All specifications are weighted by their total origination volume during the event window. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Internet Appendix for
"Does CFPB Oversight Crimp Credit?"

Andreas Fuster, Matthew Plosser and James Vickery

A Dataset construction

This section describes how we construct the bank- and loan-level datasets used for our analysis. We combine data from the following sources:

- FFIEC 031/041 (Call Report)
- FR-Y9C
- Thrift Financial Report
- CFPB Supervised Institutions Lists
- National Information Center (NIC) Attributes Data
- HMDA Loan Level data
- Avery HMDA Lender Files (concordance between HMDA ID and bank RSSD IDs)[1]
- Community Reinvestment Act Small Business Lending data

A.1 Bank panel

The first step is to assemble the sample of banks subject to analysis for each event: (i) the formation of the CFPB in July 2011, and (ii) the federal election in November 2016. The bank samples are identified as follows:

1. **Identify banks with $1-$25 billion in assets.** We identify all savings banks and commercial banks with $1bn-$25bn in total assets in the quarter prior to each event date, namely as of 2011:Q2 and as of 2016:Q3. Total assets and other bank characteristics are taken from the

[1] These files are available, for instance, at https://sites.google.com/site/neilbhutta/data.

Call Report for commercial banks and for savings banks from 2012:Q1 onwards; savings banks data prior to 2012:Q1 are drawn from the Thrift Financial Report (TFR).[2]

2. **Drop banks with parent >\$50bn and identify bank type.** We then exclude any bank controlled by a bank holding company (BHC) with total assets exceeding \$50bn.[3] We identify large BHCs by using the NIC attributes table to identify each bank's parent holding company, and then merge data on the parent BHC's consolidated assets from the FR Y-9C. We also use NIC to measure the bank's entity type. Banks are flagged as savings banks if they have an entity type of "FSB", "SAL", "SLHC", or "SSB".

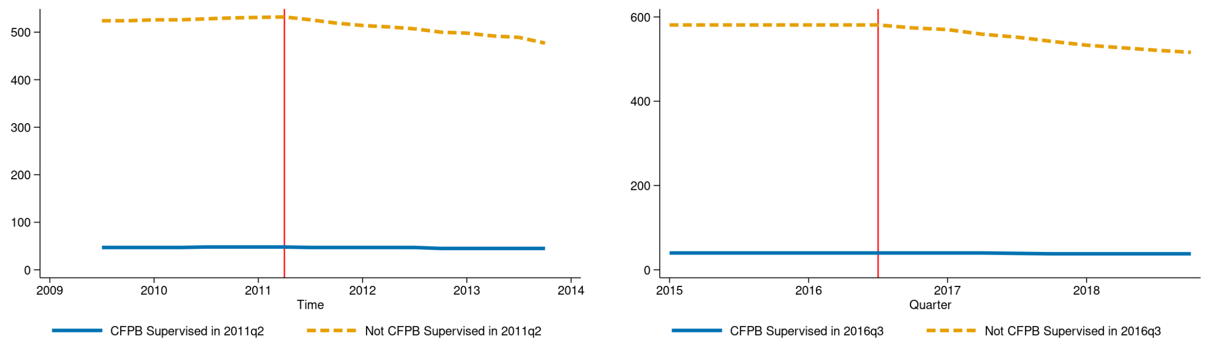
3. **Merge with identifier for CFPB oversight.** We identify which banks became subject to CFPB oversight based on quarterly lists of CFPB-supervised institutions provided on the CFPB public website. We merge these lists with the bank samples for each event by bank RSSD ID x quarter. Since the CFPB lists do not include RSSD IDs prior to 2012:Q3, for earlier quarters we fuzzy match the CFPB list to regulatory data by institution name and manually correct discrepancies. We then create a static dummy variable for whether the bank is subject to enhanced oversight. For the analysis around the formation of the CFPB, this dummy is equal to 1 for banks which are part of the CFPB's initial list of supervised depository institutions. For the analysis around the 2016 federal election, this dummy is equal to 1 for banks subject to CFPB oversight as of 2016:Q3.

These steps result in two bank $\times$ quarter panels, one for each event, including bank-level characteristics and an indicator variable for whether the firm is subject to CFPB oversight. The bank panel based on 2011:Q2 assets includes 580 entities, of which 48 are classified as subject to CFPB oversight. The panel based on 2016:Q3 assets includes 619 banks, of which 40 banks are subject to CFPB oversight. Figure IA.1 below plots the number of entities included in the two panels over time—it shows that the samples are fairly stable, although there is some post-event sample attrition, which reflects a combination of mergers, failures, or banks that converted to a different entity type (e.g., to a branch).

[2] The TFR was discontinued at the end of 2011, and savings banks began filing the Call Report in 2012:Q1. A few savings banks switch earlier, in either Q3 or Q4 of 2011, so Call Reports data are used instead for those cases.

[3] Large BHCs exceeding this \$50bn cutoff are subject to additional regulation such as the Comprehensive Capital Analysis and Review (CCAR). Midsize bank subsidiaries of large BHCs may also behave differently e.g., because they have greater access to funding via their parent.

Figure IA.1: Evolution of Bank Sample



(a) CFPB formation

(b) 2016 federal election

A.2 Merge with HMDA mortgage data

To prepare the loan-level dataset used for our analysis of mortgage lending, we first match each bank RSSD ID for the sample identified above to all applicable HMDA lender IDs by quarter, using the Avery HMDA lender files. This is a one-to-many merge which identifies the HMDA lender IDs of: (i) the bank itself if it is a HMDA filer, (ii) any of the bank's subsidiaries which are HMDA filers for that year, and (iii) other affiliates owned by same parent BHC which are HMDA-filers.[4]

The Avery lender file is also used to identify the entity type of each HMDA lender. For our main sample we exclude nonbank lenders, identified as entities with an Avery entity type other than 10 or 20.[5] We do this because nonbank lenders are subject to CFPB supervision and enforcement activities regardless of size (even if controlled by a bank <$10bn in assets), therefore there is no differential change in regulation around the $10bn size threshold for these subsidiaries. But as discussed in the main text, we also conduct robustness tests retaining these nonbank subsidiaries in the sample. [6] These nonbank entities are generally nondepository "mortgage banks" which are either subsidiaries of a depository institution in our sample or under the same holding company.

This step yields a quarterly panel of the set of HMDA filers matched to the banks in our sam-

[4]From the Avery documentation: "In the case of a HMDA filer who is a subsidiary of a bank or thrift, the HMDA filer is matched to the parent institution. If the filer is a subsidiary of a bank holding company, the filer is matched to the lead (largest) bank of the holding company."

[5]As discussed in the main text, we conduct robustness tests retaining these nonbank subsidiaries in the sample. We also test for substitution from nonbanks to banks among those banks below the $10bn asset size threshold.

[6]Avery entity types for our sample include the following: 10 - commercial banks; 11 - commercial bank subsidiary; 12 - subsidiary of a commercial bank HC; 20 - thrift institution; 21 - thrift institution subsidiary; 22 - subsidiary of a thrift holding company; 40 - independent mortgage bank; 41 - independent mortgage bank affiliated with a depository.

ple. Statistics for this sample are reported below. The first set of columns report how many banks are in the bank panel, in total and split by whether in the >$10bn treatment group. The second set of columns report how many of these banks have at least one affiliated HMDA filer (this filer is in most cases the bank itself given that we drop nonbank affiliates from our main sample). The final set of columns report the total *number* of HMDA filers, which is somewhat higher than the previous columns e.g., because of the presence of some multibank holding companies.

Table IA.1: Number of Banks and HMDA Filers by Quarter around the CFPB Creation (2011)

	No. Banks			No. Banks with HMDA Filer			No. HMDA Filers		
Quarter	CFPB	Not CFPB	Total	CFPB	Not CFPB	Total	CFPB	Not CFPB	Total
2009q3	47	524	571	41	486	527	55	545	600
2009q4	47	524	571	41	486	527	55	545	600
2010q1	47	526	573	42	493	535	72	565	637
2010q2	47	526	573	42	493	535	72	565	637
2010q3	48	528	576	42	494	536	72	567	639
2010q4	48	530	578	42	495	537	72	568	640
2011q1	48	531	579	44	488	532	52	552	604
2011q2	48	532	580	44	489	533	52	554	606
2011q3	47	526	573	44	489	533	52	554	606
2011q4	47	519	566	44	489	533	52	554	606
2012q1	47	514	561	42	476	518	55	546	601
2012q2	47	511	558	42	476	518	55	546	601
2012q3	47	507	554	42	476	518	55	546	601
2012q4	45	500	545	42	476	518	55	546	601
2013q1	45	498	543	41	451	492	49	503	552
2013q2	45	492	537	41	451	492	49	503	552
2013q3	45	489	534	41	451	492	49	503	552
2013q4	45	477	522	41	451	492	49	503	552

Table IA.2: Number of Banks and HMDA Filers by Quarter around the 2016 Federal Election

	No. Banks			No. Banks with HMDA Filer			No. HMDA Filers		
Quarter	CFPB	Not CFPB	Total	CFPB	Not CFPB	Total	CFPB	Not CFPB	Total
2015q1	40	581	621	37	554	591	43	618	661
2015q2	40	581	621	37	554	591	43	618	661
2015q3	40	581	621	37	554	591	43	618	661
2015q4	40	581	621	37	554	591	43	618	661
2016q1	40	581	621	36	548	584	44	609	653
2016q2	40	581	621	36	548	584	44	609	653
2016q3	40	581	621	36	548	584	44	609	653
2016q4	40	574	614	36	548	584	44	609	653
2017q1	40	570	610	35	502	537	39	566	605
2017q2	40	559	599	35	502	537	39	566	605
2017q3	39	552	591	35	502	537	39	566	605
2017q4	38	542	580	35	502	537	39	566	605
2018q1	38	533	571	35	475	510	39	532	571
2018q2	38	527	565	35	475	510	39	532	571
2018q3	38	521	559	35	475	510	39	532	571
2018q4	38	516	554	35	475	510	39	532	571

Note that HMDA is an annual collection and is only filed by banks that exist as independent legal entities at the end of the calendar year. Therefore there is no Avery link for banks which were acquired, failed etc. during the course of the year. Because of this selection effect, even though the size of the bank panel experiences fairly smooth attrition over time, the number of banks matched to HMDA is generally flat or nearly flat within-year, with step movements from one year to the next.

The final step is then to merge this bank panel with HMDA lender IDs to loan-level confidential-use HMDA data. Our loan level sample excludes loans which were purchased rather than originated, as well as mortgages with principal balances exceeding $5 million.

A.3 CRA data on small-business lending

To conduct the analysis outlined in Section 7, we rely on small business lending (SBL) origination data provided by the FFIEC. This data is collected by regulators to evaluate banks compliance with the CRA. We pull the following raw data from the FFIEC CRA website: (1) Transmittal Sheet, (2) Aggregate Data, and (3) Disclosure Data. Transmittal Sheets provide bank information that can be used to link the data to other regulatory datasets. Aggregate Data contains geographic-level origination information, and Disclosure Data files provide small business originations (Table ID 1-1) at the bank level, including the location of the lending activity.

We combine the data sets using respondent ID, year, and agency code. The resulting file contains the volume and number of small business loan originations at the bank-county-year level from 2004 to 2017.

A.3.1 Merge with the bank panel

In order to consider the impact of the CFPB, we merge the SBL data with the bank panel described in previous steps so we can identify which banks were CFPB supervised. To do so we:

1. **Generate list of unique banks from the bank panel.** From the bank panel, we create a list of bank RSSD IDs (subsidiary-level) and an indicator variable that flags banks exceeding $10bn in 2011:Q2 assets.
2. **Merge the list of banks with the SBL data.** We perform a many-to-one merge on RSSD IDs with the list of banks.

Of the 574 merged institutions, 61 institutions are above the $10bn threshold, and 513 institutions are below the $10bn threshold. To ensure comparability across analyses, we restrict the set of counties to a common set that appear in both the CRA and HMDA samples. Furthermore, we subset the data such that the CRA panel is balanced and the bank exists for all years in the relevant event window. In contrast to the HMDA-only analysis, balancing the panel is an important step because reporting of SBL is only required for banks with assets above roughly $1bn. As a result, natural variation in asset size results in noise in market shares, particularly for small banks.

Table IA.3: SBL Sample Summary: CFPB Formation

	No. Banks				CFPB Share	
Year	CFPB	Not CFPB	Total	Counties	Vol. (%)	Loan (%)
2010	25	170	189	1767	28.5	36.5
2011	25	170	188	1760	28.4	33.4
2012	25	170	191	1773	28.4	35.4
2013	25	170	192	1779	29	38.1

Table IA.4: SBL Sample Summary: Federal Election

	No. Banks				CFPB Share	
Year	CFPB	Not CFPB	Total	Counties	Vol. (%)	Loan (%)
2015	17	167	184	1793	18.8	24.4
2016	17	167	180	1788	18.3	24
2017	17	167	182	1792	15.9	20.4
2018	17	167	182	1793	16	20.4

Tables IA.3 and IA.4 summarize the number of banks and counties for each event period by year. Given we are restricted to annual data, we choose symmetric event windows where two years are assumed to be treated (2010, 2011 and 2015, 2016) and two years untreated (2012, 2013 and 2017, 2018). 2011 and 2016 are indicated as pre-event years given the events in question occur in the final quarter. In the final columns, we show the average county-level market share of small business lending originations by CFPB supervised banks in dollars and in loan counts.

B Additional detail on mortgage lending analysis

B.1 Robustness checks

Table IA.5 shows various robustness checks for our main loan-level regressions in Sections 4.1 and 4.3. In each case, we run these checks separately for all mortgages (columns 1-4) and FHA mortgages (columns 5-8).

All regressions presented in this table use our preferred specification, which is the same as in columns 3 of Tables 1 and 3, but apply different sample restrictions:

- In columns 1 and 5, we restrict the sample to mortgages used for home purchases (not refinancings) only.
- In columns 2 and 6, we exclude banks with less than $2.5bn in assets.
- In columns 3 and 7, we instead expand the upper end of the asset size range to $50bn.
- In columns 4 and 8, we drop banks within $2.5 bn of the $10 bn cutoff, since those may be affected by strategic considerations — in particular, banks just below the cutoff may want to reduce their lending in order to avoid crossing the threshold for tighter regulation. Such incentives are less relevant for residential mortgages, which are usually securitized (particularly for FHA loans, but also conventional mortgages securitized through the GSEs), but threshold effects may still have indirect effects on mortgage lending behavior.

Panel A shows the results for these different specifications around the formation of the CFPB in 2011. In general, the results support our conclusions that CFPB oversight had little effect on total lending but significantly reduced FHA lending. One exception is column 3, in which the coefficient becomes substantially more negative than in the baseline specification, implying an economically negative effect on *total* lending. This suggests that banks with assets between $25 and $50bn cut their lending relative to smaller ones, but they may not constitute a relevant comparison group for banks with less than $10bn of assets (and may have been affected by other regulations). Turning to the FHA sample, we see that the coefficient becomes smaller in magnitude in column 6, where lenders in the $1-$2.5bn size range are included, suggesting that these small lenders were growing their FHA lending share particularly quickly after 2011:Q2. The estimate is still statistically significant, however. The estimates are little changed from our main specification if we include banks up to $50bn in size, or exclude banks near the $10bn size threshold for CFPB oversight (column 8).

In panel B, which repeats the same exercise for the 2016 event, the coefficients are generally stable across the different samples. One exception is the effect on overall lending, which is not significant in columns 3 and 4, but this effect was relatively small to begin with.

Table IA.5: Regression of probability of loan being originated by CFPB-supervised bank – Robustness to different sample restrictions.

A. Introduction of CFPB

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All mortgages				FHA mortgages			
Post-2011Q2	-0.0104*	-0.0107**	-0.0464***	0.000597	-0.0341***	-0.0191***	-0.0593***	-0.0640***
	(0.00547)	(0.00481)	(0.00487)	(0.00779)	(0.00595)	(0.00559)	(0.00632)	(0.00741)
N	1290059	2266428	4127810	3320543	233495	199913	348172	320224
Mean Y	0.35	0.60	0.44	0.37	0.35	0.66	0.42	0.43
Loan controls	Y	Y	Y	Y	Y	Y	Y	Y
Census Tr. FE	Y	Y	Y	Y	Y	Y	Y	Y
Weighted	Y	Y	Y	Y	Y	Y	Y	Y
FHA Only	N	N	N	N	Y	Y	Y	Y
Sample	1-25bn, Purchase	2.5-25bn	1-50bn	1-7.5, 12.5-25bn	1-25bn, Purchase	2.5-25bn	1-50bn	1-7.5, 12.5-25bn

B. 2016 Federal Election

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All mortgages				FHA mortgages			
Post-2016Q4	0.0143***	0.0150***	0.00366	0.00491	0.0395***	0.0499***	0.0409***	0.0297***
	(0.00330)	(0.00378)	(0.00334)	(0.00320)	(0.00597)	(0.00819)	(0.00660)	(0.00512)
N	1846268	2112228	3046948	2344025	236284	221288	291986	229344
Mean Y	0.29	0.40	0.38	0.36	0.35	0.46	0.41	0.44
Loan controls	Y	Y	Y	Y	Y	Y	Y	Y
Census Tr. FE	Y	Y	Y	Y	Y	Y	Y	Y
Weighted	Y	Y	Y	Y	Y	Y	Y	Y
FHA Only	N	N	N	N	Y	Y	Y	Y
Sample	1-25bn, Purchase	2.5-25bn	1-50bn	1-7.5, 12.5-25bn	1-25bn, Purchase	2.5-25bn	1-50bn	1-7.5, 12.5-25bn

Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

B.2 HMDA characteristics and default risk

This section presents evidence on the relationship between HMDA loan characteristics and ex-post mortgage default. HMDA data for our sample period does not contain information on key risk indicators like borrower credit scores (e.g., FICO) or loan-to-value ratios.[7] HMDA also does not contain any information on mortgage performance after origination. For these reasons, we rely on a loan-level match between HMDA and the McDash mortgage servicing dataset (made available by the RADAR group at the Federal Reserve Bank of Philadelphia) and attempt to find loan-level risk proxies based on information that is available in HMDA. We use a sample of first-lien mortgages originated between 2005 and 2014.

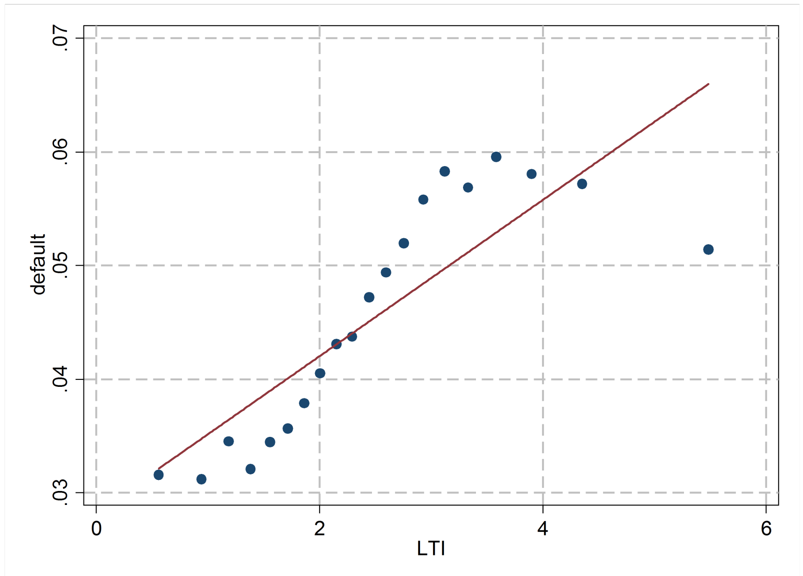
We find two variables that exhibit a particularly strong relationship with default: the ratio of mortgage loan amount to household income (LTI), and a dummy for whether the primary mortgage applicant had a co-borrower.

Figure IA.2 shows that default rates are higher for loans with an elevated LTI ratio, although the relationship is nonlinear—there is an upward-sloping relation for low to moderate LTI values, but no clear incremental relationship (or even at some point a negative relation) for LTI values exceeding three. Given this shape, we use a dummy for LTI > 3 as a measure of high loan risk.

Figure IA.3 shows that, in addition to high-LTI loans having significantly higher default across loan vintages, default is also significantly higher for loans where there is no coapplicant. The lack of a co-borrower reduces risk-sharing in the case of job loss, for example, although this relationship is also likely due in part to selection effects. For our purposes, however, we are simply interested in which variables are predictive of default from a statistical perspective; thus we use the interaction of these two variables as a proxy for elevated credit risk in our HMDA analysis.

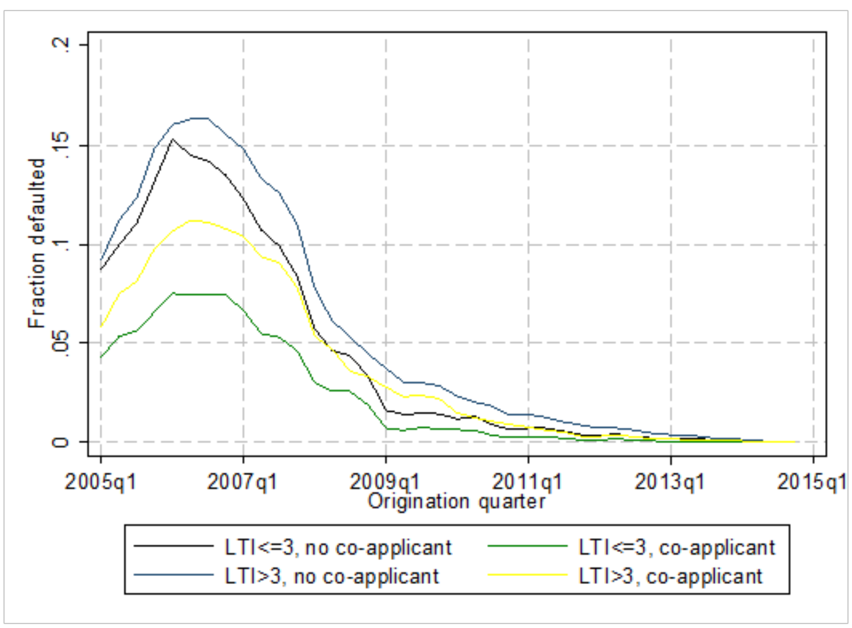
[7] Since 2018, this information is included in the HMDA data collection.

Figure IA.2: Default rate by loan-to-income (LTI) ratio



Notes: binned scatter plot of a dummy for mortgage default against loan-to-income ratio, controlling for the quarter of loan origination.

Figure IA.3: Defaults over time by loan-to-income (LTI) ratio and coapplicant status



Notes: default rates for mortgages with different characteristics, plotted against the quarter of loan origination

B.3 Alternative difference-in-differences model

As an alternative to the specifications in the main text analyzing how CFPB oversight changes the composition of lending, we also estimate models of the form:

$$loantype_{ilct} = \alpha_{ct} + \delta_l + \beta \cdot (post2011Q2_t \times CFPBsupervised_l) + \gamma purpose_{ilct} + \varepsilon_{ilct}, \tag{5}$$

where $loantype_{ict}$ is a dummy equal to 1 if mortgage i from lender l in census tract c originated in quarter t was of a particular type—either FHA, jumbo, conventional-conforming or high risk (defined as having a single loan applicant and a loan-to-income ratio $>$ 3); $post2011Q2_t$ is a dummy variable equal to 1 during the time period when the CFPB is active (2011:Q3 onwards); $CFPBsupervised_l$ a dummy equal to 1 if lender l became subject to CFPB oversight in 2011:Q3; α_{ct} is a set of census tract $\times$ quarter fixed effects; δ_l is a set of lender fixed effects, and $purpose_{ilct}$ is a set of loan purpose dummies.

Estimates of β, the coefficient of interest, are reported below. The key results are consistent with our findings in the main text. In particular, we find that becoming subject to CFPB oversight leads to a decrease in FHA lending, which is predominately offset by an increase in jumbo lending. There is no significant effect on conventional mortgage lending.

Table IA.6: Difference-in-differences analysis of changes in loan composition

	FHA	Jumbo	Conv. Conf.	No coapp. × High LTI	FHA	Jumbo	Conv. Conf.	No coapp. × High LTI
Post-2011Q2 × CFPB Sup.	-0.0148***	0.00727**	0.00255	-0.00154	-0.0127***	0.00369***	0.00276	0.00304**
	(0.00272)	(0.00285)	(0.00431)	(0.00177)	(0.00268)	(0.00143)	(0.00321)	(0.00147)
N	3506503	3506503	3506503	3506503	3506503	3506503	3506503	3506503
Mean Y	0.07	0.16	0.74	0.17	0.09	0.04	0.84	0.15
Controls + FEs	Y	Y	Y	Y	Y	Y	Y	Y
Weighted	Y	Y	Y	Y	N	N	N	N

Notes: Linear probability model. Dependent variable equal to one if mortgage is of the type indicated in the column header. All regression models include census tract × quarter fixed effects, bank fixed effects, and loan purpose dummies. In first four columns, observations are weighted by loan amount. Standard errors are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

B.4 Enlarged sample including nonbank affiliates

In our primary analysis we exclude mortgages originated by nonbank affiliates of the banks in our sample (because these nonbank affiliates are subject to CFPB oversight regardless of their size).[8] In this section we re-estimate our main results using an enlarged loan-level sample which instead retains these nonbank affiliates.

Table IA.7: Results for sample including nonbank affiliates

Dependent variable = 1 if loan is originated by a banking organization in which banks are subject to CFPB oversight

A. All mortgages

	(1)	(2)	(3)	(4)
Post-2011Q2	0.0258***	0.00384	0.000979	-0.00708**
	(0.00847)	(0.00586)	(0.00553)	(0.00357)
N	4313817	4311328	4311328	4311328
Mean Y	0.35	0.35	0.35	0.31
Loan controls	N	N	Y	Y
Census Tr. FE	N	Y	Y	Y
Weighted	Y	Y	Y	N

B. FHA mortgages

	(1)	(2)	(3)	(4)
Post-2011Q2	-0.0115*	-0.0219***	-0.0251***	-0.0213***
	(0.00687)	(0.00469)	(0.00480)	(0.00479)
N	509608	497118	497118	497118
Mean Y	0.33	0.32	0.32	0.32
Loan controls	N	N	Y	Y
Census Tr. FE	N	Y	Y	Y
Weighted	Y	Y	Y	N

Sample based on lending by banks with assets as of 2011:Q2 in the $1bn-25bn range. Loan-level sample includes mortgages originated by these banks as well as any of their nonbank affiliates. Sample period is 2010:Q1 to 2013:Q4. Only mortgages with loan amount up to $5 million are included. Loan controls include log(applicant income), log(loan amount), and indicator variables for applicant race and gender, whether the property is owner-occupied, loan purpose, loan type (conventional, FHA, VA, FSA), jumbo status, missing applicant income, and whether there is a co-applicant. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

Table IA.7 above re-estimates Table 1 from the main text, which studies the mortgage lending effects of the formation of the CFPB in mid-2011. We find directionally similar results to our primary analysis, although the estimates are smaller in magnitude. In our preferred specification

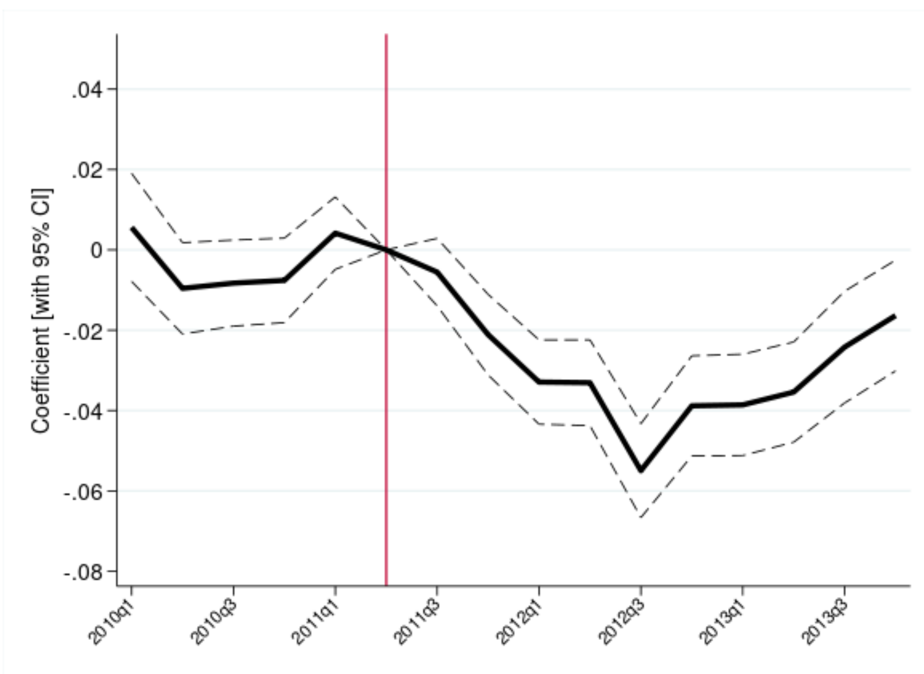
[8] Specifically we drop mortgages originated by lenders with an Avery lender type other than 10 or 20.

(column 3), the effect size of CFPB oversight on FHA lending is -0.0251, or 8% of the sample mean (still significant at the 1% level); this compares to -0.0546, or 13% of the sample mean, in our main table of results. Similar to our main findings, the effect of CFPB oversight on *total* lending is economically small and does not have a consistent sign.

It is not surprising to find smaller estimates when using the expanded sample, for two reasons: (i) nonbank lenders of all sizes are subject to CFPB oversight in the "post" period, thus we would expect attenuation when we include them; and (ii) in the main text we find evidence that small banking organizations substitute lending from nonbank to bank affiliates, as a form of regulatory arbitrage. Our main estimates incorporate such substitution, whereas these estimates do not. The results in this robustness exercise show however that these effects are not the key driver of our main results. CFPB oversight of banks is associated with a decline in FHA lending even when we examine total lending by all affiliates within the banking organization.

In the figure below we reproduce our "event study" analysis estimating the timing of changes in FHA lending. As before, these are produced by replacing the "post" dummy from the regressions with a vector of time dummies, and then tracing out the time path of these dummies, using our preferred specification which includes census tract fixed effects and loan controls.

Figure IA.4: Effects of CFPB oversight on FHA lending — Sample including nonbank affiliates



Note: Figure traces out coefficients on the vector of time dummies from a regression of CFPB-supervised lender on time dummies, census tract fixed effects and loan-level controls, with observations weighted by loan amount. 2011:Q2 coefficient normalized to zero. Based on enlarged sample including nonbank affiliates.

The dynamics of the response to CFPB oversight is similar to the corresponding figure in the main text. There is no evidence of a differential pre-trend in FHA lending prior to the formation of the CFPB. The share of lending attributable to CFPB-supervised organizations then starts declining after 2011:Q2, reaching its minimum in 2012:Q3. Lending by CFPB-supervised organizations then partially recovers, based on the point estimate, but remains below pre-CFPB levels until the end of the sample period.

C CFPB oversight and migration to nonbanks

We explored using a matching approach to examine whether CFPB oversight of banks induced substitution of FHA mortgage lending to independent nonbanks. Even though these nonbank lenders are also under the CFPB's jurisdiction, nonbanks may be less concerned about CFPB oversight because they have lower franchise value than banks and are not subject to prudential oversight or other bank regulations.

We find some evidence consistent with this substitution hypothesis, although as we show below, our results are sensitive to the specification and the set of controls used. As a result, we thus do not believe we can draw strong conclusions from this analysis, particularly in light of the challenges in appropriately matching "similar" bank and nonbank mortgage lenders due to their differences in funding and business models and the changes in the nonbank sector during this period. However, we present the results below for completeness, and perhaps as some suggestive evidence that CFPB oversight has contributed to the declining bank share of FHA lending in the period since the recovery from the Great Recession.

C.1 Matching approach

We match each bank to an independent nonbank lender with relatively similar FHA lending growth and lending volume in the pre-event period.[9] Specifically we match based on the minimum Mahalanobis distance of (i) $Log(2011Q1 + 2011Q2) - Log(2010Q1 + 2010Q2)$ and (ii) $Log(2010Q3 + 2010Q4 + 2011Q1 + 2011Q2)$. Matching is with replacement, and is implemented using the Stata package *mahapick*. FHA loan volumes are measured by summing HMDA originations for the relevant quarter. Banks are only matched if they have positive FHA lending in both the second half of 2010 and first half of 2011.

C.2 Results

We then estimate loan-level linear probability models in which the dependent variable is a dummy equal to 1 if the lender is a bank rather than a matched nonbank. For this exercise we focus on the formation of the CFPB, which occurred shortly before a period of rapid growth in nonbank lending, particularly in the FHA market (see Buchak et al., 2018). We exclude low-quality matches, applying two different thresholds for the Mahalanobis distance, either 0.1 or 0.2. Results are presented in Table IA.8.

[9] Independent nonbanks are identified by an Avery entity type of 40.

Table IA.8: CFPB oversight and the growth in nonbank FHA lending

Dependent variable: = 1 if mortgage is originated by a bank

A. Matches where Mahalanobis distance < 0.2

	(1)	(2)	(3)	(4)	(5)	(6)
Post-2011Q2	-0.0344***	-0.000151	-0.122***	-0.0269***	-0.0344***	-0.00226
	(0.00902)	(0.00232)	(0.00606)	(0.00269)	(0.00902)	(0.00237)
Post-2011Q2 × CFPB Supv.					-0.0872***	-0.0122***
					(0.0118)	(0.00448)
N	372041	358902	368733	355640	740774	731785
Mean Y	0.48	0.48	0.37	0.38	0.43	0.43
Loan controls	N	Y	N	Y	N	Y
Census Tr. FE	N	Y	N	Y	N	Y
Weighted	Y	Y	Y	Y	Y	Y

B. Matches where Mahalanobis distance < 0.1

	(1)	(2)	(3)	(4)	(5)	(6)
Post-2011Q2	-0.0345***	-0.000525	-0.0367*	0.00132	-0.0345***	-0.00128
	(0.00901)	(0.00233)	(0.0222)	(0.00590)	(0.00901)	(0.00222)
Post-2011Q2 × CFPB Supv.					-0.00214	0.00166
					(0.0250)	(0.00688)
N	369848	356631	90645	74621	460493	447744
Mean Y	0.48	0.49	0.47	0.49	0.48	0.48
Loan controls	N	Y	N	Y	N	Y
Census Tr. FE	N	Y	N	Y	N	Y
Weighted	Y	Y	Y	Y	Y	Y

Sample period is 2010:Q1 to 2013:Q4. Loan level FHA mortgage originations from HMDA. Sample includes sample banks ($1-$25bn in assets) with nonzero FHA lending in H2:2010 and H1:2011, and matched nonbanks. Excludes low-quality matches where Mahalanobis distance exceeds either 0.1 or 0.2. Mortgages with principal exceeding $5m are excluded. Loan controls include log(applicant income), log(loan amount), and indicator variables for applicant race and gender, whether the property is owner-occupied, loan purpose, loan type (conventional, FHA, VA, FSA), jumbo status, missing applicant income, and whether there is a co-applicant. Standard errors reported in parentheses are clustered by county. ***, **, * indicate statistical significance at 1%, 5%, and 10%, respectively.

In the first four columns, we split the sample by whether or not the bank is CFPB-supervised in the post-2011:Q2 period. We then estimate linear probability models for whether the mortgage is originated by a bank, as a function of a post-CFPB dummy and in columns 2 and 4 a set of loan controls and census tract fixed effects. In general the coefficients on the post-2011:Q2 dummy variables are negative and statistically significant in columns 1-4, consistent with the fact that there was an overall shift in FHA lending from banks to nonbanks over this period.

Then we test whether the shift in lending to nonbanks is larger for the $>$\$10bn asset size group which becomes subject to CFPB oversight. We do this by pooling the two samples together and estimating a model including a CFPB dummy and a CFPB $\times$ post-2011:Q2 interaction term. In panel A of the table this interaction term is negative and statistically significant, consistent with more pronounced migration of lending to nonbanks as a result of CFPB oversight. The coefficient is substantially smaller, albeit still significant, in column 6 after we include loan controls and census-tract fixed effects. However, the effect is no longer statistically significant when we further restrict the sample to banks with a close nonbank match (Mahalanobis distance < 0.1), as shown in panel B.

We conclude that there is some evidence consistent with this substitution hypothesis, although given the sensitivity of the results and the limitations of the matching exercise, we do not believe we can draw strong conclusions from this analysis.

D Bank-level analysis

CFPB oversight may have more general effects on banks' operations and profitability, beyond the specific effects on mortgage lending behavior which this paper focuses on. For instance, CFPB regulation may affect other types of consumer lending (e.g., home equity loans, auto loans or credit cards), or increase banks' non-interest expenses due to regulatory compliance costs. To investigate a broader range of potential impacts, we move to a bank-level analysis using quarterly Call Report filings by banks. We focus this analysis on the period surrounding 2011:Q3, when the CFPB became active.

We construct a sample of bank-level financial data for the period 2010:Q1 to 2013:Q4. We obtain data from Call Reports on the financial condition of commercial banks and supplement that with data from Thrift Financial Reports so that we can fill in data for thrifts during the earlier portion of our sample.[10]

Based on the combination of the two financial reports, we obtain a sample of quarterly financial data for 675 unique institutions between the range of $1bn and $25bn in assets as of 2011:Q2. As in our loan-level analysis, "post" is defined as the period from 2011:Q3 onwards, after the CFPB becomes active, and "CFPB supervised" is defined as the initial set of banks subject to CFPB oversight.

Results studying effects on asset growth, balance sheet composition, and noninterest expenses are presented below. In general our estimates are relatively noisy and are generally not statistically significant when we cluster by bank as we do here. We conclude that it is difficult to measure with confidence the effect of CFPB oversight on bank-level outcomes. We present our analysis here however, for completeness.

An additional caveat associated with this bank-level analysis is that there are other regulatory implications associated with a $10bn asset size threshold: banks are required to undertake company-run stress tests, and become subject to the Durbin Amendment, which restricts interchange fees on debit cards. As we discuss in the body of the paper, these additional regulations are unlikely to be of primary importance for mortgage origination, but might influence some of the other bank-level variables we consider below. To the extent that they do, the estimates should be viewed as an upper bound of the effect of CFPB oversight.

[10] In mid-2011, the Office of Thrift Supervision (OTS) was merged with the Office of the Comptroller of the Currency (OCC). As a result many institutions in the relevant size range went from filing Thrift Financial Reports with the OTS to Call Reports with the FDIC. All thrifts were required to file Call Reports by the first quarter of 2012. The structure of the Thrift Financial Report is similar to the Call Report, and the variables we use are defined in a generally consistent way.

D.1 Asset growth

The first outcomes we consider are asset and loan growth. If regulatory oversight by the CFPB materially increases the cost of financial intermediation, firms subject to CFPB oversight may grow more slowly after the CFPB becomes operational. To test this possibility we estimate a difference-in-difference analysis comparing asset growth for CFPB-supervised and non-supervised banks before and after 2011:Q2,

$$\Delta Y_{it} = \alpha_i + \gamma_t + \beta \cdot post2011Q2_t \times CFPBsupervised_i + \varepsilon_{it}, \tag{6}$$

where ΔY_{it} is log change of loans or assets for bank i at quarter t, $post2011Q2$ is a dummy variable indicating quarters after CFPB implementation and $CFPBsupervised$ is a dummy for those banks subject to CFPB oversight in the "post" period. The specification includes bank, α_i, and quarter, γ_t, fixed effects. The interaction term estimates how relative growth rates between treated and untreated firms change post 2011:Q2.[11] We consider equal-weighted, as well as asset weighted specifications. We also consider specifications which restrict the sample to banks with assets between \$5bn and \$25bn; this specification greatly reduces our observation count by excluding many of the smaller banks, but is arguably better identified as it compares banks of more similar size.

Results are presented in panel A of Table IA.9. Both for asset and loan growth, we consistently find a negative coefficient on the difference-in-difference term, although the coefficient is relatively small and not statistically significant. The dependent variable is measured as a quarterly log change—so for example, the coefficient of -0.00346 in column (4) means that CFPB-supervised banks grow 0.346% more slowly per quarter than non-supervised banks (post-relative-to-pre).[12]

Underlying this negative difference-in-differences coefficient, the raw mean quarterly log change in assets for the "treated" group of banks that become subject to CFPB oversight is 2.9% per quarter in the "pre" period, declining to 1.6% in the "post" period. For the non-treated group, the growth rate also declined but by a smaller amount—from 1.2% to 0.8%.[13] Therefore, one caveat regarding the results in Table IA.9 is that the higher growth of the CFPB-treated banks in the "pre" period suggests that there are some pre-existing differences between the two groups (e.g., reflecting the ongoing consolidation of the U.S. banking industry).[14] As an additional caveat, we note that the

[11] Note the uninteracted terms are unnecessary as they are accounted for by bank and time fixed effects.

[12] In unreported analysis we have also estimated versions of these regressions using winsorized changes in log assets, to minimize the effect of extreme observations due to merger events and other large shifts in asset size. Again the difference-in-difference coefficient on asset and loan growth is not statistically significant. In general the coefficient is closer to zero.

[13] These differences-in-differences in raw mean do not exactly match any of the regression coefficients, e.g., because the panel is somewhat unbalanced due to attrition in the post period.

[14] We intend to address this issue more thoroughly in a future draft; e.g., by constructing a weighted

power of the regression is relatively low (i.e., we would be unable to reject the null hypothesis of no differential trend in growth rates even for a quite economically significant point estimate).

The average effects estimated in panel A may mask significant nonlinearities close to the $10bn asset size threshold. For example, previous work by Bouwman et al. (2018) finds evidence that banks below but close to the $10bn Dodd-Frank size threshold grow more slowly post-financial crisis–their interpretation is that this group of banks is attempting to avoid the enhanced Dodd-Frank regulation associated with crossing the $10bn size threshold. To investigate these nonlinearities for our sample (which includes both commercial banks and thrifts, unlike Bouwman et al., 2018), we split the CFPB-supervised group of banks into two groups, based on whether total assets in 2011:Q2 exceed $15bn, and split the nonsupervised group based on whether assets exceed $7.5bn. We then include separate interactions for each group with the post-2011:Q2 dummy (so that there are four groups in total, rather than two).

Results are shown in panel B. Note that the coefficients measure the post-2011:Q2 change in growth rates for each group *relative* to non-CFPB-supervised banks with less than $7.5bn in assets (the omitted group, given that the regressions include a vector of time dummies). We do not find any evidence of discontinuities in growth rates near the threshold. Most notably, and perhaps surprisingly, we find evidence that non-supervised banks with assets between $7.5bn and $10bn actually grow *more* quickly than smaller non-supervised banks. In unreported regressions we find that this seems to be explained by a higher propensity to engage in merger events. It may be that banks just below the threshold want to cross the threshold by a significant margin, in order to spread fixed compliance costs or other noninterest expenses across a wider asset base.

These results seem different to Bouwman et al. (2018), who find that banks in this size range do grow significantly more slowly. This may reflect differences in methodology. Specifically, our sample includes both commercial banks and thrifts, and we compare behavior pre- and post-2011:Q2, whereas Bouwman et al. (2018) compare banks pre- and post-crisis. The summary statistics in Table 3 of Bouwman et al. also suggest that indirectly treated banks just below the $10bn Dodd-Frank threshold do grow more quickly than banks above the threshold, and about the same as smaller banks significantly below the $10bn threshold, in the post-crisis period.

In Table IA.11, we further test if these patterns differ depending on whether a bank is heavily focused on retail lending. To do so, we use specifications where our main interaction term of interest is further interacted with an indicator for whether a bank was in the top quartile of the ratio of retail loans over total assets over the four quarters prior to the CFPB beginning operations. The resulting triple interaction coefficients are not statistically significant, but are positive across the specifications corresponding to panel A of Table IA.9, meaning that if anything, high-retail

synthetic control group which matches the CFPB-treated group on growth rates in the "pre" period.

banks subject to CFPB oversight grow relatively *more* strongly in the post-2011:Q2 period. This is opposite to what one would expect if CFPB oversight constrained these banks' growth—but potentially consistent with the alternative story where these banks aim to grow more to spread compliance costs across more assets.

D.2 Balance sheet composition

While overall asset and loan growth does not seem to have been significantly negatively impacted by the introduction of CFPB oversight, banks may have shifted lending away from retail loan products which are marketed to consumers and toward business oriented products, thereby shifting the composition of loans held on balance sheet. To consider this hypothesis, we estimate a version of equation (6) in which the dependent variable is the retail loan share—defined as the value of all consumer-type loans on the bank's balance sheet, including residential mortgages, HELOCs, credit cards, automobile loans and other consumer loans, as a percentage of total assets. Results are presented in panel C of Table IA.9. In column (1), we find weak evidence that CFPB-supervised firms shifted their loan portfolios away from retail products. The coefficient implies retail share is on average 1.5% lower for supervised firms post 2011:Q2 relative to the non-CFPB banks. This result is statistically significant at the 10% level. However, when we consider alternative specifications that put less emphasis on the smaller banks in the sample, the results are weaker. When we weight by assets (column 2), the coefficient suggests the retail share falls by 1.1%, but the results is not statistically significant at standard levels. Similarly, dropping the banks with less than $5bn leads to a much smaller coefficient that is far from statistically significant (column 3). In sum, while the point estimates are negative, there is insufficient evidence to conclude with confidence that CFPB-supervised banks have significantly pivoted away from retail loans. This "stock" evidence seems roughly consistent with our "flow" evidence from mortgage originations presented earlier.

D.3 Noninterest expenses

The final set of bank-level analyses considers bank noninterest expense ratios. CFPB oversight may have resulted in greater compliance costs. To test this possibility we again use the empirical specification in equation (6); instead of assets or loans as the dependent variable we construct several expenses ratios to determine if particular costs for CFPB-supervised firms increased. We consider total noninterest expense, and then two subcategories of noninterest expense: compensation and other noninterest expense. For each category of expense we construct two ratios: one scaled by assets and one scaled by revenues (net interest margin plus noninterest income). We winsorize ratios at the 2.5% level to reduce the influence of extreme outliers on the results.

In Table IA.10, we find varying coefficients, some positive and some negative, but none are statistically different from zero. There are three panels each of which reflects one of our empirical specifications; equal-weighted results are summarized in panel A, asset weighted in panel B, and the \$5bn-\$25bn asset range (equal-weighted) in panel C. Columns (1)-(3) contain ratios scaled by assets. For each category of noninterest expense, the magnitude of the coefficient generally suggests that expense ratios are lower for CFPB-supervised institutions. None of the results are statistically different from zero at the 10% level. The negative magnitudes are much closer to zero in panel C when we exclude the firms below \$5bn in assets.

Ratios may vary because of expenses (numerator) or the scaling factor (denominator). In columns (4)-(6) we consider an alternative ratio in which we scale by revenue rather than assets. These coefficients suggest a positive relationship between CFPB supervision and expenses, albeit none of the coefficients are statistically significant. The positive coefficients are consistent across all three categories: total noninterest expense, compensation expense and other noninterest expenses. The largest magnitudes and *t*-statistics can be found in panel C, when the smallest firms are excluded. However, taken together these results do not suggest a strong or consistent relationship between CFPB supervision and expense ratios.

In Table IA.11, we further find that there are no significant differential effects of CFPB oversight on noninterest expenses across banks that are more vs. less focused on retail lending. However, we do find that total expenses and the non-compensation part grew significantly more strongly across all retail-focused banks in our sample (not just those subject to CFPB oversight) in the post-2011 period. This suggests that retail lending may have become more cost-intensive independent of CFPB oversight (perhaps reflecting increased aversion to legal risk from guarantor or investor lawsuits, or mortgage "put-backs"; e.g., Buchak et al., 2018; D'Acunto and Rossi, 2020; Fuster et al., 2017; Gissler et al., 2016; Hartman-Glaser et al., 2014).

Table IA.9: Difference-in-difference: Asset and loan growth, and balance sheet composition

A. Growth	Δ log(assets)			Δ log(total loans)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post * CFPB supv.	-0.00846	-0.00648	-0.0171	-0.00346	-0.000532	-0.00366
	(0.0138)	(0.0125)	(0.0169)	(0.0135)	(0.0138)	(0.0243)
Observations	8372	8372	1409	8306	8306	1397
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighted by 2011q2 Assets	No	Yes	No	No	Yes	No
$5B-25$B in Assets	No	No	Yes	No	No	Yes

B. Interactions near 10bn threshold	Δ log(assets)			Δ log(total loans)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post * CFPB supv. * >$15bn	0.00682	0.00673	-0.00127	0.00846	0.0131	0.0249
	(0.00704)	(0.00814)	(0.0161)	(0.00853)	(0.0116)	(0.0339)
Post * CFPB supv. * <$15bn	-0.0145	-0.0133	-0.0197	-0.00748	-0.00417	0.00766
	(0.0196)	(0.0219)	(0.0261)	(0.0189)	(0.0223)	(0.0390)
Post * NonSupv. * >$7.5bn	0.0177***	0.0180**	0.00917	0.0263***	0.0316***	0.0426
	(0.00627)	(0.00722)	(0.0157)	(0.00892)	(0.0115)	(0.0341)
Observations	8372	8372	1409	8306	8306	1397
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighted by 2011q2 Assets	No	Yes	No	No	Yes	No
$5B-25$B in Assets	No	No	Yes	No	No	Yes

C. Asset composition	% retail loans		
Post * CFPB Supv.	-1.535*	-1.168	-0.303
	(0.928)	(0.890)	(1.760)
Observations	8882	8882	1492
Time FEs	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes
Weighted by 2011q2 Assets	No	Yes	No
$5B-25$B in Assets	No	No	Yes

Table contains results estimates the difference-in-differences for CFPB-supervised banks relative to unsupervised banks after 2011:Q2. Columns 1, 2, 4, and 5 are based on banks in the $1-25bn range. Columns 3 and 6 are based on banks in the $5-25bn range. Retail and non-retail loan shares are calculated relative to total loans. Weighted specifications are based on 2011:Q2 assets. Standard errors in parentheses clustered by entity. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table IA.10: Difference-in-difference: Expense ratios

Panel A	NIE/Assets (1)	Comp./Assets (2)	Other/Assets (3)	NIE/Rev. (4)	Comp./ Rev. (5)	Other/Rev. (6)
Post*CFPB Supv.	-1.298	-0.680	-0.793	0.972	0.272	0.267
	(2.077)	(0.928)	(1.148)	(1.051)	(0.609)	(0.738)
Observations	8952	8952	8952	8949	8949	8949
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes

Panel B Asset Weighted	NIE/Assets (1)	Comp./Assets (2)	Other/Assets (3)	NIE/Rev. (4)	Comp./ Rev. (5)	Other/Rev. (6)
Post*CFPB Supv.	-1.764	-1.121	-0.779	1.298	0.407	0.202
	(2.097)	(0.996)	(1.152)	(1.061)	(0.636)	(0.769)
Observations	8952	8952	8952	8949	8949	8949
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes

Panel C $5B-25$B Assets	NIE/Assets (1)	Comp./Assets (2)	Other/Assets (3)	NIE/Rev. (4)	Comp./ Rev. (5)	Other/Rev. (6)
Post*CFPB Supv.	0.0837	-0.150	-0.0197	1.690	0.735	0.335
	(2.488)	(1.151)	(1.448)	(1.523)	(0.808)	(1.051)
Observations	1505	1505	1505	1504	1504	1504
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table contains results estimates the difference-in-differences for CFPB-supervised banks relative to unsupervised banks after 2011:Q2. Panels A and B are based on banks in the $1-25bn range. Panel B is asset weighted based on 2011:Q2 assets. Panel C is based on banks in the $5-25bn range. Asset ratios are reported in bps. Revenue (NIM+NII) are reported in %. Ratios are winsorized at the 2.5% tails. Weighted specifications are based on 2011:Q2 assets. Standard errors in parentheses clustered by entity. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table IA.11: Asset growth, composition and expenses: Triple-interaction with share of retail and consumer loans

A. Growth	Δ log(assets)			Δ log(total loans)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*CFPB*High Consumer	0.0116	0.0166	0.0184	0.00683	0.00906	0.0122
	(0.0224)	(0.0199)	(0.0301)	(0.0215)	(0.0211)	(0.0367)
Post*CFPB Supv.	-0.0136	-0.0137	-0.0244	-0.00666	-0.00510	-0.00856
	(0.0205)	(0.0186)	(0.0240)	(0.0203)	(0.0200)	(0.0333)
Post*High Consumer	0.00868	0.0118	0.00896	0.00687	0.0119	0.00667
	(0.00543)	(0.00746)	(0.0204)	(0.00683)	(0.0110)	(0.0297)
Observations	8372	8372	1409	8306	8306	1397
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
Weighted by 2011q2 Assets	No	Yes	No	No	Yes	No
$5B-$25B in Assets	No	No	Yes	No	No	Yes

B. Asset composition	% retail loans		
Post*CFPB*High Consumer	0.364	0.561	3.631
	(1.944)	(2.147)	(5.357)
Post*CFPB Supv.	-1.390	-1.002	-0.895
	(1.186)	(1.027)	(1.607)
Post*High Consumer	-2.426**	-3.153*	-6.344
	(0.969)	(1.631)	(5.060)
Observations	8882	8882	1492
Time FEs	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes
Weighted by 2011q2 Assets	No	Yes	No
$5B-$25B in Assets	No	No	Yes

C. Expenses	NIE/Ass	Comp./Ass	Other/Ass	NIE/Rev.	Comp./ Rev.	Other/Rev.
Post*CFPB*High Consumer	0.161	0.552	3.712	-0.0343	-0.777	1.428
	(4.943)	(2.208)	(2.379)	(2.368)	(1.322)	(1.675)
Post*CFPB Supv.	-2.249	-1.318	-2.428**	1.096	0.700	-0.580
	(2.045)	(1.151)	(1.071)	(1.148)	(0.798)	(0.709)
Post*High Consumer	3.603***	0.00436	2.754***	1.801*	-0.140	2.305***
	(1.335)	(0.786)	(0.779)	(0.952)	(0.533)	(0.663)
Observations	8952	8952	8952	8949	8949	8949
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
Weighted by 2011q2 Assets	Yes	Yes	Yes	Yes	Yes	Yes
$5B-$25B in Assets	No	No	No	No	No	No

High consumer is a dummy equal to 1 for banks in the top quartile of ownership of retail & consumer loans in the year leading up to the opening of the CFPB. Table contains results estimates the difference-in-differences for CFPB-supervised banks relative to unsupervised banks after 2011:Q2, and for post * high-consumer, as well as a triple interaction term post * CFPB * high consumer. Ratios are winsorized at the 2.5% tails. Weighted specifications are based on 2011:Q2 assets. Standard errors in parentheses clustered by entity. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.